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I. THE CHEMISTRY OF THE FUTURE.

IN selecting this subject we lay no claim either to the gift of second sight or to the possession of a clearer view of the future development of Science than may be the lot of our contemporaries. Nor is it our purpose to write an imitation of Winterl's celebrated "Prodromus," in which enthusiasts may find whatever they think proper. We seek merely to call the attention of our fellow-workers, and especially of students, to certain researches which hold out great promise, and which, if duly followed up, will undoubtedly have a most important influence on the very foundations of chemistry.

It must be confessed that, as regards these very foundations, the alphabet of the science, our knowledge is not merely limited, but unsatisfactory in the highest degree. Look at our "elements." Most chemists quietly accept them as ultimate facts, and work with them—or perhaps play with them—more or less judiciously, quietly waiving all inquiry into their nature and their origin. Are they absolutely elementary bodies, distinct from the beginning, and resolvable neither into each other nor into any forms of matter still unknown? Or are they compounds, elementary in the mere relative acceptation that their decomposition is a task not within our present knowledge and power. Have we any evidence of their simplicity other than what our grandfathers had of the supposed elementary character of potash and soda prior to Davy's great discovery? If compounds, are they all of the same order, or are some of them, perhaps, resolvable into the remainder, whilst these, in turn, consist of ultimate—or at least ulterior—bodies, as yet undiscovered? If simple, are they like the wheels and pinions

of a machine, or the parts of a dissected puzzle, definite in their number and purpose, and all necessary to a given result? Or are they a mere fragmentary and accidental group of objects with which we build our *Lagerungs formel*, just as children construct their sand forts and shell grottoes on the shore? What would be the impression of a man of inquiring mind if for the first time made acquainted with the so-called elements and their leading properties? He would see a list of some sixty bodies, from which, he is told, all things visible or tangible—all matter, in short—are compounded. But why their number should be between 60 and 70, rather than between 30 and 35 or between 160 and 170, no reason is given or even conjectured. Some of them, he is informed,—such as oxygen, hydrogen, nitrogen, silicon, aluminium, and sulphur,—are exceedingly abundant. Others, on the contrary,—as vanadium, thallium, indium, cæsium, and gallium, occur only in minute traces. Some are widely distributed, and others concentrated in comparatively few localities. Turning to their properties he finds equal difficulties. Here he will see a number of "elements" identical, or at least closely approaching, in their atomic weights. There, on the contrary, he finds wide gaps. Thus betweeen cerium (140) and erbium (178) there intervenes not an element. A smaller blank is found between bismuth (208) and thorium (231), between tungsten (184) and osmium (195), between zinc (65) and arsenic (75); whilst, on the contrary, between 86 and 96 we number six elements, and between 195 and 200 five (see Table I.). These gaps may, indeed, be possibly filled up by the discovery of some rare element, but there is also the possibility that new discoveries may fall in the more thickly-filled parts of the series. With those properties of the elements which cannot as yet be exhibited in a numerical series the case is very similar. There are groups showing a close approximation in their characters and behaviour. There are elements which stand comparatively isolated.

How are all these facts to be explained on the theory of elements primordially distinct? Popular opinion, here as elsewhere, takes refuge in teleology. The elements in their respective proportions and in their distribution exist, as we find them, for the sake of man's convenience. We demur to this hypothesis. Look at sulphur; where existing in quantity the very key with which we unlock the treasure-house of Nature—no less essential to the chemical technologist than is iron to the engineer; but where occurring in small quantities, what a source of evil! Take the case of

coal: there is probably no purpose to which we apply it—in metallurgy, in the gas-manufacture, in the generation of steam, or in domestic economy—where its value is not seriously impaired, and where nuisance and danger are not occasioned by the presence of a small percentage of sulphur. Far too little and far too equally distributed to be worth, or even capable of, extraction, it is yet far too much to be inactive, or other than formidable. Or, again, take phosphorus: absolutely necessary to our existence, and, of course, most valuable when met with in quantities and forms capable of utilisation; but when found, as it so frequently is, in traces accompanying iron, it is the source of incalculable loss and annoyance. Or, consider arsenic: on teleological principles, surely, a substance so poisonous, and yet at the same time so useful for certain technological purposes, should have been concentrated in some few places. But we find it very widely disseminated, present to a serious extent in most iron pyrites, and thus contaminating sulphuric and hydrochloric acids, and through them a variety of other chemicals.

On the other hand, we look at the case of gold: had it been more abundant, its low affinity for oxygen and sulphur and its power of resisting the action of organic acids would have rendered it exceedingly important, both for manufacturing and domestic purposes. Were gold as plentiful as copper the latter metal would be entirely banished from dye- and print-works.

As for the rarer metals, if they exert any function it escapes, as a rule, our notice, whilst not a few of them, if common, would have been highly important. Thus on teleological principles we see nothing to explain, either the number, the relative amount, or the local distribution of the elements of many of them, scarcely, even, the very existence.

We turn, therefore, to another point of view. The array of the elements cannot fail to remind us of the general aspect of the organic world. It shows us the same gaps due to our old acquaintance, the "missing link," or rather to its non-appearance. In both cases we see certain groups well filled up, whilst other forms stand isolated. Both display species that are common and species that are rare. Hence it seems natural, in the one instance as in the other, to view existing forms not as originally present, but as the outcome of a process of evolution, or, if the reader likes the expression, the residue after a "struggle for existence." Certain forms not in harmony with the present general

conditions have disappeared; certain others have maintained themselves, indeed, but only on a limited scale; whilst a third class are abundant because circumstances have been favourable to their formation and preservation. The analogy, it must be remembered, is not the closest, and must not be pushed too far. There is of necessity a wide difference between species composed of lifeless beings, incapable of growth, reproduction, and decay, and species consisting of living organisms. From the nature of the case there cannot occur in the "elements" any distinction corresponding to that between living and fossil organic forms. The "stone book" tells us nothing of extinct elements. Nor would we for a moment suggest that any of our present elements, however rare, is disappearing; that any new element is in the course of formation; or that the properties of such as exist are in course of modification. All such changes, in as far as they took place at all, must have been confined to the pre-geological epoch—to the time when our earth, or rather the matter of which it consists, was in a state very different from its present condition. Making, however, every allowance for these distinctions, if evolution is the law of the universe manifested in the heavenly bodies, in organic individuals, and in organic species, we shall probably recognise it also—though under an especial aspect—in those elements from which stars and organisms are, in the last resort, compounded.

But where is the evidence that the elements have been formed by the "expansion" * of some few antecedent principles, at present hidden, or perhaps from one only primordial kind of matter? Were our attention confined to our own planet we might perhaps find little either to verify or to confute our hypothesis. But if we institute a comparative examination of the chemical composition of different heavenly bodies, as revealed to us by the spectroscope, evidence will be found. Into its nature and value we will now proceed to enquire.

On an examination of the spectrum of our sun we recognise certain elementary bodies as decidedly present in his atmosphere. These are sodium, calcium, barium, magnesium, iron, chrome, nickel, copper, zinc, hydrogen, aluminium, titanium, manganese, and possibly strontium, cadmium, and cobalt. The following terrestrial elements, however, appear to be absent:—Gold, silver, mercury, rubidium, tin, potassium, lead, antimony, arsenic, lithium,

* *See* Quarterly Journal of Science, January, 1877, p. 26.

silicon, glucinum, cerium, lanthanum, didymium, ruthenium, iridium, palladium, and platinum. The remaining terrestrial bodies may possibly still be discovered in the sun, as there are many lines not yet identified. On the other hand, it is quite possible that some of these lines may indicate the presence in the sun of elements other than our own, and possibly of a simpler nature.

The spectra of the stars again differ from those of the sun. Meteorites have not, to our knowledge, been subjected to as careful and thorough spectroscopic examination as they deserve; but on ordinary chemical analysis they are found to contain some of the elements present in the earth, but wanting in the sun, viz., silicon, potassium, tin, and probably antimony, arsenic, and lead.

Thus we see that nineteen well-characterised terrestrial elements are absent in the sun. To explain this fact three hypotheses may be proposed:—We may assume that each sun or planet is a body independent in its origin and materials, and not therefore necessarily containing the same chemical elements as its neighbours. This view is, of course, irreconcilable with the nebular hypothesis, and though it might have met with general acceptance in the beginning of the century, we think it will fail to command the assent of the most judicious physicists of the day.

Or, secondly, we may conceive that the missing elements, from some of their attributes, are more likely to escape observation than are others. It is quite possible that simple bodies present in the sun in exceedingly small proportions —bodies of very high specific gravity or of sparing volatility—might not be recognised in the spectrum; but on going carefully over the list of the missing elements we can scarcely pronounce such considerations admissible. Among these elements we find one, silicon, which on our earth ranks among the most plentiful bodies. We perceive, indeed, certain bodies of a decidedly fixed character,—such as gold, platinum, and iridium,—but along with these occur such signally volatile substances as potassium, mercury, arsenic, and antimony. The heaviest bodies are included, such as gold and platinum, but also lithium and potassium, which are remarkably light. Perhaps we may say that the missing elements have, on the average, higher atomic weights than those bodies found present in the sun.* Yet the former list includes glucinum and lithium, which rank, in this respect,

* This circumstance, we think, favours the hypothesis of the compound nature of our present elements.

among the lowest of the metallic elements. We do not, therefore, see any reason why the above nineteen elements, if present in the sun, should elude our researches.

The third hypothesis seeks to account for the absence of certain terrestrial elements in the solar atmosphere by assuming that at the temperature there existing such elements would be dissociated, or rather that they could never be formed. This supposition may be tested by the following methods :—

a. Physicists conclude that all the fixed stars are by no means equal in the energy of the thermic phenomena which they display. Some emit a pure and intense white light, and seem to be hotter than our sun ; others, of a deep yellow or red hue, are supposed to be in a more advanced stage of their career, and to have greatly cooled down. If this third hypothesis is correct the white stars will contain none of the elements absent in our sun, whilst some of those distinguished in him will in them probably be missing. With the redder—or, generally speaking, the more highly-coloured —stars, on the contrary, some of the elements absent in our sun ought to be recognised, and none which are found in him ought to be wanting. There is also, as we have already hinted, a further consideration : if any of our elements cannot exist in the sun or in the stars, it becomes highly probable that in the solar and stellar spectra there will be indications of bodies, not existing *as such*, on our globe, but which are the materials from which such missing elements are composed. Mr. Lockyer, to whose researches in this direction we shall shortly refer, considers that a series of photographs of stellar spectra will afford valuable information in regard to the constitution of certain substances now regarded as elementary. We may here remark that the discovery, in the sun or in the stars, of any element not found in the earth, is, with the means at our disposal, scarcely conceivable. The spectroscope shows us certain lines : some of these we identify with the characteristic lines of terrestrial elements ; others we fail to identify, and therein we reach, for the present, our limits. How can we, from out of the mass of unidentified lines, select one as characteristic of *x*, another as belonging to *y*, and a third as peculiar to *z*, when these bodies are strictly, to us, unknown quantities ?

b. The next method for the verification of the above hypothesis is a direct attack of some of the supposed elements —perhaps preferably some of the nineteen found wanting in the sun—from the most promising point of view. There may, perhaps, be little hope that any of our present simple

bodies can be successfully assailed by any chemical process "pure simple," that is, by bringing it into reaction with any agent calculated to abstract from it some one of its unknown constituents, setting another (or others) at liberty. Probably an intense temperature would be the most likely means of approaching the conditions existing in the sun. A practical difficulty is here to be encountered or evaded. It is perfectly possible that at extreme temperatures some element might be dissociated,—the result may have been obtained already,—but that on cooling re-combination has taken place, the apparent result being *nil*. Mitscherlich and Plücker have observed, some years ago, that in the hydrogen flame iodine showed broad bands, whilst in the far higher temperature produced by the electric spark it displayed merely bright lines. Hence, if it be correct that bright lines are characteristic of elementary bodies, whilst bands are produced only by compounds, it may be inferred that iodine when heated in the spark becomes dissociated, and resolves itself into some elements not yet detected.

But the substance whose elementary character has been of late most strongly called in question is calcium. Some time ago Mr. Lockyer declared his conviction that it is not a simple substance, but that the H lines in its spectrum are due to two elementary bodies of which it is composed. In the photographs of the spectrum of *α* Lyræ taken by Dr. Huggins only one of these H lines is present—a fact which leads to the inference that only one of the constituents of the metal calcium can be present in the star *α* Lyræ. This point, therefore, requires a most careful examination. It is interesting to find that researches of a totally different character have led an eminent chemist of the present day to entertain grave doubts concerning the homogeneous nature of this same metal. Whether the phenomena observed indicate that calcium is really a compound which under certain circumstances suffers a partial decomposition, or whether they merely prove that under the name of calcium we confound two—or perhaps more—metals, identical in their atomic weights, and closely approximating in their respective properties, as is the case with cobalt and nickel, it would be premature to decide. Nor can we anticipate the gentleman concerned by indicating the class of reactions which he is now submitting to a rigorous examination. Should these suspicions be confirmed the result will be in either case interesting, and may probably throw an unexpected light upon certain questions in geology, mineralogy, and physiology. Still, in the second alternative, instead of

finding ourselves on the road towards a decrease in the number of our elements, we shall have another, or perhaps two more, to account for and to harmonise.

We now pass to the consideration of researches having no direct connection with the question we have just been handling, but which still indirectly testify in favour of the compound nature of our so-called elements. A law has been proposed which exhibits these "elements" not as a haphazard assemblage of independent bodies, whose number, properties, and atomic weights might have been other than we find them, but as a definite series, or rather group of series, whose members bear to each other relations somewhat similar to the successive grades—*e.g.*, of oxidation—of some one supposed element. The law in question, though it does not remove the teleological difficulties inherent in the respective quantities and the distribution of the simple bodies, solves, at any rate, some of the hitherto unanswered questions which they have put before us. It does more; it enables us to declare not merely that a link is wanting in the series, but to foretell with tolerable accuracy not alone its atomic weight and its specific gravity, but even certain of its reactions. The prevision of phenomena not yet observed has been rightly declared by methodologists to be one of the principal distinctions between a science, in the strict sense of the term, and a mere accumulation of unorganised knowledge. We still hear mention, from time to time, of the splendid triumph achieved by Astronomy, when Leverrier —having from certain observed facts deduced the existence of a planet as yet unknown—was able to calculate its distance, its mass, its orbit and probable position, and when his announcements were found verified on the telescopic examination of the part of the heavens indicated. Such a fulfilment of his forecasts was a verification of astronomical science perfectly intelligible to the outside public. Prof. Mendeleeff, by the application of his "periodic law," was able to foretell distinctly, in 1869, the properties of a metal then unknown, to which he gave the name of "eka aluminium." On the evening of August 27th, 1875, M. Lecoq de Boisbaudran, being engaged with the examination—chemical and spectroscopic—of a blende from the mine of Pierrefitte, discovered a new metal, to which he has given the name of "gallium," in honour of his country. He does not appear to have been acquainted with the predictions of M. Mendeleeff, which, till their startling verification, had not by any means attracted the attention which they undoubtedly merit. The more thoroughly, however, M. Lecoq

de Boisbaudran succeeded in purifying the body which he had obtained, and in accurately determining its properties and reactions, the more closely did it approximate to M. Mendeleeff's "eka aluminium." It must be distinctly understood that the prediction had not been couched in loose generalities like the prophecies of the late Francis Moore, Physician. In 1869 M. Mendeleeff wrote as follows:—"Its atomic weight will be El=68; its oxide, El_2O_3; its salts will present the formula ElX_3. Thus its (only ?) chloride will be $ElCl_3$, yielding on analysis 39 per cent of metal and 61 of chlorine, and will be more volatile than $ZnCl_2$. Its sulphide, El_2S_3, or oxysulphide, $El_2(S,O)_3$, will be precipitable by sulphuretted hydrogen and insoluble in ammonium sulphide.

"The metal will be easily obtained by reduction; its specific gravity will be 5·9, consequently its atomic volume will be 11·5; it will be almost fixed and fusible at a low temperature. It will not become oxidised in contact with the atmosphere, and at a red-heat it will decompose water. The pure metal melted will be slowly attacked by the acids and alkalies. The oxide, El_2O_3, will have the specific gravity 5·5, or thereabouts; it should be soluble in strong acids, form an amorphous hydrate insoluble in water, but soluble in acids and alkalies. The oxide will form neutral and basic salts, $El_2(OH,X)_6$, but not acid salts; its alum, $ElK(SO_4)_2 12H_2O$, will be more soluble than the corresponding salt of aluminium and less crystallisable. The basic properties of El_2O_3 being more decided than those of Al_2O_3, and less than those of ZnO, it will be precipitable by carbonate of baryta. The volatility, as well as the other properties of the saline compounds of El, being the mean between those of aluminium and those of indium, it is probable that the metal will be discovered by means of spectrum analysis, as was the case with indium and thallium."

This very definite account may be read in "Liebig's Annalen" (Supplement-Band viii., p. 133, 1871), and it has indeed been most strikingly confirmed by the properties of the metal as observed by M. Lecoq de Boisbaudran. We must particularly bear in mind that this accord between prediction and observation is evidently becoming more complete as the new metal is obtained in larger quantities and in a state more closely approximating on purity. The celebrated French chemist, indeed, remarks—"Supposing the forecasts of M. Mendeleeff verified altogether, I should have been led to seek for gallium in the precipitates formed by ammonia, and not, as I have done, in the ammoniacal

solutions. In fact, the properties of the hypothetical metal ought to 'present the mean between those of aluminium and indium,' metals whose oxides are almost completely insoluble in ammonia." Yet in a footnote he very materially qualifies this deliverance. He there states—"Oxide of indium is generally considered almost insoluble in ammonia, a property which is utilised in its separation. As for alumina, its solubility in ammonia, though slight, is sensible. It remains to be seen whether the great delicacy of the spectral reaction of gallium, and the minuteness of the quantities upon which I have operated, may not have caused me to over-rate the relative insolubility of gallic oxide in ammonia." We cannot here help pointing out that the solubility of hydrated alumina in ammonia is sufficiently great to vitiate an analysis, even for technological purposes, unless certain well-known precautions are observed. Hence gallia may still be taken up by ammonia to an extent amply sufficient for spectroscopic purposes, even if considerably less soluble in that medium than is alumina.

M. Lecoq de Boisbaudran further observes that without the particular method followed in the present investigation neither the theories of M. Mendeleeff nor his own would have, for a long time, led to the discovery of gallium. With all due deference we must submit that this point is utterly beside the question. If certain theories enable us to foretell correctly the properties of a metal as yet undiscovered, their value is established, and whether the ultimate discovery of such metal was due to the prediction is a mere secondary consideration.

M. Mendeleeff has also announced the probable existence of another metal, to which he gives the name of "eka-silicium," Es=72, forming an oxide EsO_2. Its properties ought to be intermediate between those of silicium and tin, and it is to be especially sought for among arseniferous and titaniferous minerals or residues. To the discovery of this metal—if metal it may be called, since even tin is relegated among the non-metallic bodies by chemists whose opinions are entitled to respect—we must look forward with anxious interest, not so much for its own sake as for the light which it must throw upon the theory in question.

We must now proceed to an exposition of the law which, in one instance at least, we have seen so signally verified. M. Mendeleeff sets out with a brief reference to the labours of those chemists who have preceded him in this line of enquiry. Gladstone, Cooke, Dumas, Pettenkofer, have all pointed out that the atomic weights of certain groups of the

elements stand in a simple regular ratio to each other. Thus in the calcium group, taking the old atomic weights, we find a progressive increase of approximately 24, the other properties undergoing a correspondingly progressive change. In the sulphur group—sulphur, selenium, and tellurium—the increase of atomic weight on the present scale is approximately 47, with again a progressive modification of properties. Similar developments, which need not here be particularised, have been demonstrated in the potassium, the chlorine, and the phosphorus groups. It has also been noticed, if we are not mistaken, that—although no definite ratio be discoverable—the atomic weights of all the common elements are low, and those of the rarer high. It appears, further, that only the elements of low atomic weight enter into the composition of organised beings. With the very limited exception of copper (Cu = 63) found in the chocolate nut, in the blood of certain crustaceans, and in the feathers of the touraco, and of zinc (Zn = 65) in the ash of a pansy, iron (Fe = 56) has the highest atomic weight of the organic elements.* Again, the elements with high atomic weights, beginning with vanadium (V = 51) and chromium (Cr = 52), with perhaps the single exceptions of manganese and iron, may be regarded as poisonous whenever they exist in a soluble condition. But although such generalisations have not been wanting, there has been no attempt at bringing all the apparently independent groups into harmonious connection. M. Mendeleeff considers that the regular dependence of the properties on changes of the atomic weight appears most clearly on the consideration of dissimilar elements, by the study of which he was led, in 1869, to the discovery of his "periodic law." This law he expressed in the following words:—"The properties of simple bodies, the constitution of their combinations, as well as the properties of the latter, are periodic functions of the atomic weights of the elements." There is one term here so generally misunderstood and misapplied, not merely by persons of good general education, but even by scientific writers, that the meaning of M. Mendeleeff's law will scarcely be understood at first sight. The word "period" is commonly used to signify any portion of

* It may be said that we are using the term "organic element," or "organogen," in a manner different from its usual acceptation. We are perfectly aware of this difference; but we submit that every element really assimilated by plant or animal, and not merely lodged in its tissues as a foreign and hostile intruder,—*e.g.*, mercury in the bones of a votary of blue pill and calomel—has the right to be considered an "organogen." Lead (Pb = 207) has been found in the metallic state in the intestinal canal of insects but there is no evidence whatever in support o its assimilation.

time which we may wish to particularise; but if we remember its derivation we see that it means a "journey round," and that it is applicable only to such portions, of time or of anything else, as exhibit some series of changes tending first in a certain direction and then returning to, or at least towards, the point of departure. Thus a year is legitimately a "period," because in it certain phenomena—astronomical, meteorological, and organic—go through a circle of changes which necessarily ends where it began; but a stretch of 10, or 20, or 100 years cannot be called a period, unless we can discover in it some phenomenon which has its increase and decrease, or its recurrence in such a term of years. In like manner, if we take a group of animals, of minerals, or of elementary bodies, and find that in them some one attribute increases and then decreases again to its former condition, or suffers any other cyclical variation, we may call such a group a "period," and the change in question "periodic." We trust that none of our readers will feel aggrieved at being thus reminded of what doubtless most of them are aware, since a correct understanding on this point is absolutely necessary for the intelligibility of Mendeleeff's law. At the same time we should be very happy to find some word incapable of being misunderstood.

We now turn to Table I., which exhibits the known elements arranged in the arithmetical order of their atomic weights. In the second and third columns we find all whose weights range from 7 to 36. Here we perceive that the characters of the elements change gradually and regularly with alternating magnitudes of the atomic weights. These changes are periodic, taking place in both columns in the same manner, so that the corresponding members are analogous. If we compare respectively—

Li	Be	B	C	N	O	Fl
Na	Mg	Al	Si	P	S	Cl

we must admit that there is, between the members of each of these pairs, a resemblance, which in the cases of carbon and silicon, fluorine and chlorine, is especially striking. We notice, further, that the atomic weight of each member of the first group differs from that of the corresponding member of the second almost exactly by one and the same number, viz., 16. A correspondence of this latter nature, indeed, can no longer be traced between the corresponding members of the remaining columns. If, however, we add the atomic weight of an element in column 3 to that of its representative in column 5, and divide by 2, we obtain an

approximation to the atomic weight of the corresponding member of column 4.

Returning to the second and third series, we find that the corresponding degrees of each have the same atomicity. Only the last four members of each column can combine with hydrogen, forming respectively RH_4, RH_3, RH_2, RH. Thus we see that the power of uniting with hydrogen, absent in the first members of each series, becomes high in the middle and declines again to the end. The permanence or instability of their compounds, their acidity and power to exchange hydrogen for metals, change according to the same gradation. Thus HCl is a well-marked acid, H_2S a much feebler acid, H_3P can no longer be called an acid, and in H_4Si there is no residue of the acid character. In the corresponding members of the first column, again, HFl is manifestly acid, H_2O neutral, H_3N basic, and H_4C neither acid nor basic.

All the members of the second column combine with oxygen, forming salifiable bodies, as—

$$Na_2O,\ Mg_2O_2(MgO),\ Al_2O_3,\ Si_2O_4(SiO_2),\ P_2O_5,\ S_2O_6(SO_3),\ Cl_2O_7;$$

two atoms of the element thus combining with a number of atoms of oxygen, increasing from 1 to 6. This arrangement corresponds with the decrease of basicity and the increase of acidity. At the beginning of the series is a decided base, soda; at the end a powerful acid, the chloric; and in the middle is alumina, which may be either a feeble base or a feeble acid.

A regular connection may be traced, not merely in the combining numbers, but in other chemical and physical characteristics. At the beginning of the first, second, third, and fourth series we find in each case bodies unmistakably metallic; at the opposite end come non-metallic substances, whilst those which stand on the boundary between these two classes take an intermediate position.

Again, if we consider the same set of elements with reference to specific gravity and atomic volume, we find periodic changes. Thus in specific gravity we have—

$$Na=0{\cdot}97,\ Mg=1{\cdot}75,\ Al=2{\cdot}67,\ Si=2{\cdot}49,\ P=1{\cdot}84.$$
$$S=2{\cdot}06,\ Cl\ (liquid)=1{\cdot}33.$$

Here the numbers, as will be seen, increase up to aluminium, and then decrease again, save that sulphur is anomalous, and has a higher specific gravity than phosphorus, as the

theory would require. With the atomic volumes the periodic changes are in the opposite direction; there is first a decrease and then an increase:—

$$Na=24,\ Mg=14,\ Al=10,\ Si=11,\ P=16,\ S=16,\ Cl=27.$$

In their oxides, also, the gradation of specific gravity and atomic volume is to be traced. The specific gravities are—

$$Na_2O=2{\cdot}8,\ Mg_2O_2=3{\cdot}7,\ Al_2O_3=4{\cdot}0,\ Si_2O_4=2{\cdot}6,\ P_2O_5=2{\cdot}7,\ S_2O_6=1{\cdot}9.$$

Here again there is a rise, succeeded by a fall. The series of atomic volumes is—

$$Na_2O=22,\ Mg_2O_2=22,\ Al_2O_3=25,\ Si_2O_4=45,\ P_2O_5=55,\ S_2O_6=82.$$

Here is progressive increase without a return to the original number.

Speaking of alloys, M. Mendeleeff remarks that phosphorus and sulphur have not quite lost the metallic character of the elements at the beginning of the series, since the phosphides and sulphides have still the aspect of alloys. This feature is quite lost in the chlorides.

Turning to another series we find the atomic weights increasing as follows:—

$$Ag=108,\ Cd=112,\ In=110,\ Sn=118,\ Sb=122,\ Te=125,\ I=126.$$

Here again we have a decided metal, silver, at the head of the group; an equally decided non-metallic body, iodine, at the end; and in the middle an element, tin, whose metallic character—paradoxical as it may sound—has been questioned on totally different grounds, and which, according to M. Mendeleeff's scheme, should bear to silicon the same analogy which antimony does to phosphorus, tellurium to sulphur, or iodine to chlorine. The foretold—but as yet undiscovered—element, eka-silicium, is in this case to bridge over the gap, as in the three parallel instances is done by arsenic, selenium, and bromine.

In the silver group (No. 7) we find the specific gravities regularly decreasing as follows:—

$$Ag=10{\cdot}5,\ Cd=8{\cdot}6,\ In=7{\cdot}4,\ Sn=7{\cdot}2,\ Sb=6{\cdot}7,\ Te=6{\cdot}2,\ I=4{\cdot}9.$$

Further instances of "small series," each consisting of seven members, known or unknown, are the following:—

No. 4 . .	K	Ca	—	Ti	V	Cr	Mn
No. 5 . .	Cu	Zn	—	—	As	Se	Br
No. 6 . .	Rb	Sr	—	Zr	Nb	Mo	—

Thus M. Mendeleeff considers that all the functions by which the dependencies of the properties of the elements on the atomic weights are expressed appear as periodic. The properties change in accordance with the increasing atomic weights, and are then repeated in a new period with the same regularity as in the former. If we examine his scheme we must admit that it brings admitted relations into a very prominent light. Such groups as fluorine, chlorine, bromine, and iodine; as sulphur, selenium, and tellurium; as nitrogen, phosphorus, arsenic, and antimony; as calcium, strontium, and barium; or as potassium, rubidium, and cæsium,—though their respective members are placed in different series,—fall into positions which well agree with their respective analogies. Even the trinity of the commercial world—gold, silver, and copper—fall in the same horizontal line across the series; gold being a representative of its companion metals in a higher series. A further study of Table II. will bring to light many more curious instances of such representation, which we submit lend a powerful support to M. Mendeleeff's arrangement.

As a matter of course the mere arranging the elements in one arithmetical series would thrust these analogies totally out of sight. As completely would they be hidden if the series were made to consist of any other number of elements. M. Mendeleeff's classification is shown in two tables taken from the translation of his memoir in Liebig's "Annalen." He admits that not all the known elements can be introduced into the small series. Thus in Table I. we see iron, cobalt, nickel, ruthenium, rhodium, palladium, osmium, iridium, and platinum forming an appended eighth group, concerning which the author remarks that they are all grey, all very sparingly fusible, their fusibility increasing in each line from iron to nickel, from ruthenium to palladium, and again from osmium to palladium. Between the corresponding members of the even and the odd small series (except the two first) there is a distinct difference, whilst the corresponding members of the even and again of the odd, series show a close analogy.

Table I.—Elements in Small Series.

Series.	First Group. — R_2O.	Second Group. — RO.	Third Group. — R_2O_3.	Fourth Group. RH_4 RO_2.	Fifth Group. RH_3 R_2O_5.	Sixth Group. RH_2 RO_3.	Seventh Group. RH R_2O_7.	Eighth Group. (R_2H) (RO_4).			
1	1 H	—	—	—	—	—	—				
2	Li 7	Be 9	B 11	C 12	N 14	O 16	F 19				
3	23 Na	24 Mg	27 Al	28 Si	31 P	32 S	35 Cl				
4	K 39	Ca 40	? 44	Ti 48	V 51	Cr 52	Mn 55	Fe 56	Co 59	Ni 59	Cu 63
5	(63 Cu)	65 Zn	68 ?	72 ?	75 As	78 Se	80 Br				
6	Rb 85	Sr 87	Yt 88	Zr 90	Nb 94	Mo 96	? 100	Ru 104	Rh 104	Pd 106	Ag 108
7	(108 Ag)	112 Cd	113 In	118 Sn	122 Sb	125 Te	127 I				
8	Cs 133	Ba 137	? Di 138	Ce 140	—	—	—	Os 195	Ir 197	Pt 198	Au 199
9	—	—	—	—	—	—	—				
10	—	—	Er 178	? La 180	Ta 182	W 184	? 190	—	—	—	—
11	(199 Au)	200 Hg	204 Tl	207 Pb	208 Bi	—	—				
12	—	—	—	Th 231	—	U 240	—	—	—	—	—

Table II.—Elements in Great Series.

I.	II.	III.	IV.	V.	VI.	VII.	VIII.
			Potassium = 39	Rubidium = 86	Cæsium = 133	—	—
			Calcium = 40	Strontium = 87	Barium = 137	—	—
			—	Yttrium = 88	Didymium = 138	Erbium = 178	—
			Titanium = 48	Zirconium = 90	Cerium = 140	Lanthanum = 180	Thorium = 231
			Vanadium = 51	Niobium = 94	—	Tantalum = 182	—
			Chromium = 52	Molybdenum = 96	—	Tungsten = 184	Uranium = 240
			Manganese = 55	—	—	—	
			Iron = 56	Ruthenium = 104	—	Osmium = 195	—
			Cobalt = 59	Rhodium = 104	—	Iridium = 197	—
			Nickel = 59	Palladium = 106	—	Platinum = 198	—
			Copper = 63	Silver = 108	—	Gold = 199	—
			Zinc = 65	Cadmium = 112	—	Mercury = 200	—
			—	Indium = 113	—	Thallium = 204	—
			—	Tin = 118	—	Lead = 207	—
			Arsenic = 75	Antimony = 122	—	Bismuth = 208	—
			Selenium = 78	Tellurium = 125	—	—	—
			Bromine = 80	Iodine = 127	—	—	—

Typical.

Hydrogen = 1	Lithium = 7	Sodium = 23	
	Beryllium = 9·4	Magnesium = 24	
	Boron = 11	Aluminium = 27	
	Carbon = 12	Silicon = 28	
	Nitrogen = 14	Phosphorus = 31	
	Oxygen = 16	Sulphur = 32	
	Fluorine = 19	Chlorine = 35	

Thus if we recur to the four series above mentioned :—

No. 4	. .	K	Ca	—	Ti	V	Cr	Mn
No. 5	. .	Cu	Zn	—	—	As	Se	Br
No. 6	. .	Rb	Sr	—	Zr	Nb	Mo	—
No. 7	. .	Ag	Cd	In	Sn	Sb	Te	I

Here it is at once evident that there a decided analogy prevails between the corresponding members of Nos. 4 and 6, and again of Nos. 5 and 7. But between Nos. 4 and 5, 5 and 6, or 6 and 7, there is no such analogy.

Table II. shows the elements as arranged in "great series." In Series I. stands hydrogen alone. This reminds us of a remark made lately by Prof. F. Guthrie, that very generally the commonest bodies are the most anomalous. Series II. contains the seven elements from lithium to fluorine inclusive, these two first groups being designated "typical." Series III. contains also seven elements. Series IV. contains fourteen bodies, which, when the three blanks indicated by M. Mendeleeff have been filled up, as one of them already is by the discovery of gallium (eka-aluminium), will be raised to seventeen.

This "great series" consists of the two "small series," No. 4 and No. 5, with three of the non-serial elements—iron, cobalt, and nickel—interposed between them. The great series No. 5 is composed of sixteen known elements, and a blank space is left for a body not yet discovered. The small series here embraced are Nos. 6 and 7, and again three of the non-serial elements—ruthenium, rhodium, and palladium—are interpolated. The great series VI. is very imperfect, comprehending only four elements, which are placed parallel with the first four bodies in Nos. IV. and V. Great series VII. is more perfect; it contains parts of two small series, and again three non-serial elements—osmium, indium, and platinum. Here, however, we are struck with a difficulty: between the atomic weights of tungsten (W = 184) and osmium (Os = 195) there is the gap of 11. Now if M. Mendeleeff concluded that two undiscovered elements, eka-aluminium and eka-silicium, must exist to fill up the interval of 10 between Zn = 65 and As = 75, in great series IV., why may we not with equal right calculate on the discovery here also of *two* unknown bodies? But should this be hereafter found to be the fact, the correspondence between the members of Series V. and VII. will be deranged. Gold will occupy the position corresponding not with silver, but with cadmium; mercury

will figure as the representative of indium; thallium, of tin; lead, of antimony; and bismuth, of tellurium; changes which will not commend themselves to the approval of chemists. Altogether we find that the difference between the successive atomic weights, in parts of the series where M. Mendeleeff does not suspect—or at least does not indicate—an absent element, ranges from 1 to 5. What will then become of the interesting harmonies which the scheme exhibits if some undiscovered element should crop up to fill some of the larger of these intervals? We certainly do not find that the author shows any reason why such an event should be improbable. M. Mendeleeff himself declares that the Periodic Law cannot be harmonised with the Atomic theory without inverting known facts.

It will, we think, be evident to the reader that—splendidly as the discovery of gallium has fulfilled the deductions drawn from the law under consideration—the subject is still in its infancy, and that future research is abundantly needed to confirm, to modify, or to extend. We should suggest to chemists and physicists a course parallel to what we have recommended to biologists in the case of the Darwinian hypothesis. We would say—Take up the law provisionally, and work with it. One of the most essential steps is the search for "eka-silicium," conducted, as the author proposes, by the spectroscopic examination of titaniferous minerals and residues. The contents of certain sealed papers which M. Lecoq de Boisbaudran has deposited with the Institute, and in which he has explained his special ideas on the classification of the elementary bodies, must be awaited with interest. Readers who would wish to pursue this subject further will find detailed information in the "Journal de la Soc. Chimique Russe" (i., p. 60); in Liebig's "Annalen" (Supplement-Band viii., p. 183, 1871); and, if they understand the Russian language, in M. Mendeleeff's work, "Foundations of Chemistry" (vol. ii.). As far as the "periodic law" may be considered established, it seems to us most decidedly to contradict the hypothesis of some sixty primordially distinct bodies, and to exhibit our present elements as products of the evolution of something to us yet unknown. For that something it must be the task of chemists to search.

II. ON THE PROBABLE ORIGIN AND AGE OF THE SUN.

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THE total annual amount of radiation from the whole surface of the sun is 8340×10^{30} foot-pounds. To maintain the present rate of radiation it would require the combustion of about 1500 lbs. of coal per hour on every square foot of the sun's surface; and were the sun composed of that material it would all be consumed in less than 5000 years. The opinion that the sun's heat is maintained by combustion cannot be entertained for a single moment. Mr. Lockyer has suggested that the elements of the sun are, owing to its excessive temperature, in a state of dissociation, and some have supposed that this fact might help to explain the duration of the sun's heat. But it must be obvious that, even supposing we were to make the most extravagant estimate of the chemical affinities of these elements, the amount of heat derived from their combination could at most give us only a few thousand years additional heat. Under every conceivable supposition the combustion theory must be abandoned.

It is now generally held by physicists that the enormous store of heat possessed by the sun could only have been derived from gravitation. For example, a pound of coal falling into the sun from an infinite distance would produce by its concussion more than 6000 times the amount of heat that would be generated by its combustion. It would, in fact, amount to upwards of 65,000,000,000 foot-pounds—an amount of energy sufficient to raise 1000 tons to a height of $5\frac{1}{2}$ miles.

There are two forms in which the gravitation theory has been presented: the first, the meteoric theory, propounded by Dr. Meyer; and the second, the contraction theory, advocated by Helmholtz. The meteoric theory of the sun's heat has now been pretty generally abandoned for the contraction theory advanced by Helmholtz. Suppose, with Helmholtz, that the sun originally existed as a nebulous mass, filling the entire space presently occupied by the solar system, and extending into space indefinitely beyond the

outermost planet. The total amount of work in foot-pounds performed by gravitation in the condensation of this mass to an orb of the sun's present size can be found by means of the following formula given by Helmholtz :—

$$\text{Work of condensation} = \frac{3}{5} \cdot \frac{r^2 M^2}{Rm} \cdot g.$$

M is the mass of the sun, m the mass of the earth, R the sun's radius, and r the earth's radius. Taking—

$$M = 4230 \times 10^{27} \text{ lbs.}, \; m = 11{,}920 \times 10^{21} \text{ lbs.},$$
$$R = 2{,}328{,}500{,}000 \text{ feet, and } r = 20{,}889{,}272 \text{ feet},$$

we have then, for the total amount of work performed by gravitation in foot-pounds,

$$\frac{3}{5} \cdot \frac{(20{,}889{,}272{\cdot}5)^2 \times (4230 \times 10^{27})^2}{2{,}328{,}500{,}000 \times 11{,}920 \times 10^{21}}$$
$$= 168{,}790 \times 10^{36} \text{ foot-pounds.}$$

The amount of heat thus produced by gravitation would suffice for 20,237,500 years.

The conclusions are based upon the assumption that the density of the sun is uniform throughout. But it is highly probable that the sun's density increases towards the centre, in which case the amount of work performed by gravitation would be something more than the above.

At this point, in reference to the age of our globe, Geology and Physics are generally supposed to come into direct antagonism. For if it be true, as physicists maintain, that gravitation is the only possible source from which the sun could have derived its store of energy, then the sun could not have maintained our globe at its present temperature for more than about 20 millions of years. "On the very highest computation which can be permitted," says Prof. Tait, "it cannot have supplied the earth, even at the present rate, for more than about fifteen or twenty million years."* The limit to the age of the sun's heat must have limited the age of the habitable globe. All the geological history of the globe would necessarily be comprehended within this period. If the sun derived its heat from the condensation of its mass, then it could not possibly be more than about twenty million years since the beginning of the Laurentian period. But twenty million years would be considered by most geologists to represent only a comparatively small portion of the time which must have elapsed since organic life began on our globe.

* Recent Advances in Physical Science, p. 175.

It is true that the views which formerly prevailed amongst geologists, in regard to the almost unlimited extent of geological time, have of late undergone very considerable modifications; but there are few geologists, I presume, who would be willing to admit that the above period is sufficient to comprehend the entire history of stratified rocks.

It is the facts of denudation which most forcibly impress the mind with a sense of immense duration, and show most convincingly the great antiquity of the earth.

We know unquestionably that many of the greatest changes undergone by the earth's crust were produced, not by convulsions and cataclysms of nature, but by those ordinary agencies that we see at work every day around us, such as rain, snow, frost, ice, chemical action, &c. Valleys have not been produced by violent dislocations, nor the hills by upheavals, but both have been carved out of the solid rock by the silent and gentle agency of chemical action, frost, rain, ice, and running water. In short, the rocky face of our globe has been moulded into hill and dale, and ultimately worn down to the sea-level by means of these apparently trifling agents, not merely once or twice, but probably dozens of times over during past ages. Now when we reflect that with such extreme slowness do these agents perform their work that we might, if we could, watch their operations from year to year, and from century to century, without being able to perceive that they make any sensible impression, we are necessitated to conclude that geological periods must be enormous. The utter inadequacy of a period of 20 million years for the age of our earth is demonstrable from the enormous thickness of rock which is known to have been removed off certain areas by denudation. I shall now briefly refer to a few of the many facts which might be adduced on this point.

One plain and obvious method of showing the great extent to which the general surface of the country has been lowered by denudation is furnished, as is well known, by the way in which the inequalities of surface produced by faults or dislocations have been effaced. It is quite common to meet with faults where the strata on the one side have been depressed several hundreds—and in some cases thousands—of feet below that on the other, but we seldom find any indications of such on the surface, the inequalities on the surface having been all removed by denudation. But in order to effect this a mass of rock must have been removed equal in thickness to the extent of the dislocation. The following are a few examples of large faults:—

The great Irwell fault, described by Prof. Hull,* which stretches from the Mersey west of Stockport to the north of Bolton, has a throw of upwards of 3000 feet.

Some remarkable faults have been found by Prof. Ramsay in North Wales. For example, near Snowdon, and about a mile E.S.E. of Beddgelert, there is a fault with a downthrow of 5000 feet; and in the Berwyn Hills, between Bryn-mawr and Post-gwyn, there is one of 5000 feet. In the Aran Range there is a great fault, designated the Bala fault, with a downthrow of 7000 feet. Again, between Aran Mowddwy and Careg Aderyn the displacement of the strata amounts to no less than from 10,000 to 11,000 feet.† Here we have evidence that a mass of rock, varying from 1 mile to 2 miles in vertical thickness, must have been denuded in many places from the surface of the country in North Wales.

The fault which passes along the east side of the Pentlands is estimated to have a throw of upwards of 3000 feet.‡ Along the flank of the Grampians a great fault runs from the North Sea at Stonehaven to the estuary of the Clyde, throwing the Old Red Sandstone on end sometimes for a distance of 2 miles from the line of dislocation. The amount of the displacement, Prof. Geikie‖ concludes, must be in some places not less than 5000 feet, as indicated by the position of occasional outlyers of conglomerate on the Highland side of the fault.

The great fault crossing Scotland from near Dunbar to the Ayrshire coast, and which separates the Silurians of the South of Scotland from the Old Red Sandstone and Carboniferous tracts of the North, has been found, by Mr. B. N. Peach, of the Geological Survey,§ to have in some places a throw of fully 15,000 feet. This great dislocation is older than the Carboniferous period, as is shown by the entire absence of any Old Red Sandstone on the south side of the fault, and by the occurrence of the Carboniferous Limestone and Coal-measures lying directly on the Silurian rocks. We obtain here some idea of the enormous amount of denudation which must have taken place during a comparatively limited geological epoch. So vast a thickness of Old Red Sandstone could not, as Mr. Peach remarks, "have ended originally where the fault now is, but must have swept southwards over the Lower Silurian uplands. Yet these thousands of feet of sandstones, conglomerates, lavas, and

* Mem. Geol. Survey of Lancashire, 1862.
† Mem. Geol. Survey of Great Britain, vol. iii.
‡ Memoir to Sheet 32, Geol. Survey Map of Scotland.
‖ Nature, vol. xiii., p. 390.
§ Explanation to Sheet 15, Geol. Survey Map of Scotland.

tuffs were so completely removed from the south side of the fault previous to the deposition of the Carboniferous Limestone series and the Coal-measures that not a fragment of them is anywhere to be seen between these latter formations and the old Silurian floor." This enormous thickness of nearly 3 miles of Old Red Sandstone must have been denuded away during the period which intervened between the deposition of the Lower Old Red Sandstone and the accumulation of the Carboniferous Limestone.

Near Tipperary, in the south of Ireland, there is a dislocation of the strata of not less than 4000 feet,* which brings down the Coal-measures against the Silurian rocks. Here 1000 feet of Old Red Sandstone, 3000 feet of Carboniferous Limestone, and 800 feet of Coal-measures have been removed by denudation off the Silurian rocks. Not only has this immense thickness of beds been carried away, but the Silurian itself on which they rested has been eaten down in some places into deep valleys several hundreds of feet below the surface on which the Old Red Sandstone rested.

Faults to a similar extent abound on the Continent and in America, but they have not been so minutely examined as in this country. In the Valley of Thessolon, to the north of Lake Huron, there is a dislocation of the strata to the extent of 9000 feet.†

In front of the Chilowee Mountains there is a vertical displacement of the strata of more than 10,000 feet.‡ Prof. H. D. Rogers found in the Appalachian coal-fields faults ranging from 5000 feet to more than 10,000 feet of displacement.

There are other modes than the foregoing by means of which geologists are enabled to measure the thickness of strata which may have been removed in places off the present surface of the country, into the details of which I need not here enter. But I may give a few examples of the enormous extent to which the country, in some places, has been found to have been lowered by denudation.

Prof. Geikie has shown‖ that the Pentlands must at one time have been covered with upwards of a mile in thickness of Carboniferous rocks which have all been removed by denudation.

In the Bristol coal-fields, between the River Avon and the Mendips, Prof. Ramsay has shown§ that about 9000 feet of

* Jukes's and Geikie's Manual of Geology, p. 441.
† Geology of Canada, 1863, p. 61.
‡ Safford's Geology of Tennessee, p. 309.
‖ Mem. to Sheet 32, Geol. Survey of Scotland.
§ "Denudation of South Wales." Memoirs of Geol. Survey, vol. i.

Carboniferous strata have been removed by denudation from the present surface.

Between Bendrick Rock and Garth Hill, South Glamorganshire, a mass of Carboniferous and Old Red Sandstone, of upwards of 9000 feet, has been removed. At the Vale of Towy, Caermarthenshire, about 6000 feet of Silurian and 5000 feet of Old Red Sandstone—in all about 11,000 vertical feet—have been swept away. Between Llandovery and Aberaeron a mass of about 12,000 vertical feet of the Silurian series has been removed by denudation. Between Ebwy and the Forest of Dean, a distance of upwards of 20 miles, a thickness of rock varying from 5000 to 10,000 feet has been abstracted.

Prof. Hull found* on the northern flanks of the Pendle Range, Lancashire, the Permian beds resting on the denuded edges of the Millstone Grit, and these were again observed resting on the Upper Coal-measures south of the Wigan coal-field. Now, from the known thickness of the Carboniferous series in this part of Lancashire, he was enabled to calculate approximately the quantity of Carboniferous strata which must have been carried away between the period of the Millstone Grit and the deposition of the Permian beds, and found that it actually amounted to no less than 9,900 feet. He also found in the Vale of Clitheroe, and at the base of the Pendle Range, that the Coal-measures, the whole of the Millstone grit, the Yoredale series, and part of the Carboniferous Limestone, amounting in all to nearly 20,000 feet, had been swept away—an amount of denudation which, as Prof. Hull remarks, cannot fail to impress us with some idea of the prodigious lapse of time necessary for its accomplishment.

In the Nova Scotia coal-fields one or two miles in thickness of strata have been removed in some places.†

It may be observed that, enormous as is the amount of denudation indicated by the foregoing figures, these figures do not represent in most cases the actual thickness of rock removed from the surface. We are necessitated to conclude that a mass of rock equal to the thickness stated must have been removed, but we are in most cases left in uncertainty as to the total thickness which has actually been carried away. In the case of a fault, for example, with a displacement of (say) one mile, where no indication of it is seen at the surface of the ground, we know that on one side of the fault a thickness of rock equal to one

* Quart. Journ. Geol. Soc., vol. xxiv., p. 323.
† LYELL'S Student's Manual, chap. 23.

mile must have been denuded, but we do not know how much more than that may have been removed. For anything which we know to the contrary hundreds of feet of rock may have been removed before the dislocation took place, and as many more hundreds after all indications of dislocation had been effaced at the surface.

But it must be observed that the total quantity of rock which has been removed from the *present* surface of the land is evidently small in proportion to the total quantity removed during the past history of our globe. For those thousands and thousands of feet of rock which have been denuded were formed out of the waste of previously existing rocks, just as these had been formed out of the waste of yet older rock-masses. In short, as a general rule, the rocks of one epoch have been formed out of those of preceding periods, and go themselves to form those of subsequent epochs.

In many of the cases of enormous denudation to which we have referred, the erosion has been effected during a limited geological epoch. We have, for example, seen that upwards of a mile in thickness of Carboniferous rock has been denuded in the area of the Pentlands. But the Pentlands themselves, it can be proved, existed as hills, in much their present form, before the Carboniferous rocks were laid down over them; and as they are of Lower Old Red Sandstone age, and have been formed by denudation, they must consequently have been carved out of the solid rock between the period of the Old Red Sandstone and the beginning of the Carboniferous age. This affords us some conception of the immense lapse of time represented by the Middle and Upper Old Red Sandstone periods.

Again, in the case of the great fault separating the Silurians of the south of Scotland from the Old Red Sandstone tracts lying to the north, a thickness of the latter strata of probably more than a mile, as we have seen, must have been removed from the ground to the south of the fault before the commencement of the Carboniferous period. And again, in the case of the Lancashire coal-fields, to which reference has been made, nearly two miles in thickness of strata had been removed in the interval which elapsed between the Millstone Grit and the Permian periods.

As we are enabled, from geological evidence, to form some rough estimate of the extent to which the country in various places has been lowered by sub-aërial denudation during a given epoch, it is evident that we should have a means of arriving at some idea of the length of that epoch, did we

know the probable rate at which the denudation took place. If we had a means of forming even the roughest estimate of the probable average rate of sub-aërial denudation during past ages, we should be enabled thereby to assign approximately an inferior limit to the age of the stratified rocks. We could then tell, at least, whether the amount of sub-aërial denudation known to have been effected during past geological ages could have been accomplished within 20 million years or not, and this is about all with which we are at present concerned. And if it can be proved that a period of 20 millions of years is much too short to account for the amount of denudation known to have taken place, then it is certain that the gravitation theory cannot explain the origin and source of the sun's heat.

A very simple and obvious method of determining the present mean rate of sub-aërial denudation was pointed out several years ago,* viz., that the rate of denudation must be equal to the rate at which the materials are carried off the land into the sea. But the rate at which the materials are thus abstracted is measured by the rate at which sediment is carried down by our rivers. Consequently, in order to determine the present rate of sub-aërial denudation, we have only to ascertain the quantity of sediment annually carried down by the river systems.

Very accurate measurements have been made of the quantity of sediment carried down into the Gulf of Mexico by the River Mississippi, and it is found to amount to 7,474,000,000 cubic feet. The area drained by the river is 1,224,000 square miles. Now 7,474,000,000 cubic feet removed from 1,224,000 square miles of surface is equal to 1-4566th of a foot off the surface per annum, or 1 foot in 4566 years. The specific gravity of the sediment is taken at 1·9, and that of the rock at 2·5; consequently the amount removed is equal to 1 foot of rock in about 6000 years. For many reasons there are few rivers better adapted for affording us a fair average of the rate of sub-aërial denudation than the Mississippi. In reference to the above I may here quote the words of Sir Charles Lyell:—"There seems," he says, "no danger of our over-rating the mean rate of waste by selecting the Mississippi as our example, for that river drains a country equal to more than half the continent of Europe, extends through 20 degrees of latitude, and therefore through regions enjoying a great variety of climate, and some of its tributaries descend from mountains of great height. The

* Phil. Mag., May, 1868; Feb., 1867. Climate and Time, chap. 20. See also Trans. Geol. Soc. of Glasgow, vol. iii.

Mississippi is also more likely to afford us a fair test of ordinary denudation, because, unlike the St. Lawrence and its tributaries, there are no great lakes in which the fluviatile sediment is thrown down and arrested on its way to the sea." *

Rough estimates have been made of the sediment carried down by some eight or ten European rivers; and although those estimates cannot be depended upon as being anything like accurate, still they show that it is extremely probable that the European continent is being denuded at about the same rate as the American.

I think we may safely assume, without the risk of any great error, that the average rate of sub-aërial denudation during past geological ages did not differ much from the present. The rate at which a country is lowered by sub-aërial denudation is determined† not so much by the character of its rocks as by the sedimentary carrying power of its river systems. And this again depends mainly upon the amount of rain-fall, the slope of the ground, and the character of the soil and vegetation covering the surface of the country. And in respect of these we have no reason to believe that the present is materially different from the past. No doubt the average rain-fall during some past epochs might have been greater than at present, but there is just as little reason to doubt that during other epochs it might have been less than now. We may therefore conclude that about one foot of rock removed from the general surface of the country in 6000 years may be regarded as not very far from the average rate of denudation during past ages.

But some of the cases we have given of great denudation refer to comparatively small areas, and others to beds which form anticlinal axes, and which, as is well known, denude more rapidly than either synclinal or horizontal beds. We shall therefore—to prevent the possibility of over-estimating the length of time necessary to effect the required amount of denudation—assume the rate to have been double the above, or equal to one foot in 3000 years.

To lower the country one mile by denudation would therefore require, according to the above rate, about 15 million years; but we have seen that a thickness of rock more than equal to that must have been swept away since the Carboniferous period. For even during the Carboniferous period itself more than a mile in thickness of strata in many places was removed. Again, there can be no doubt whatever that

* Student's Manual of Geology, p. 91 (second edition).

† *See* Climate and Time, p. 334.

the amount of rock removed during the Old Red Sandstone period was much greater than one mile; for we know perfectly well that over large tracts of country nearly a mile in thickness of rock was carried away between the period of the Lower Old Red Sandstone and the Carboniferous epoch. Further, all geological facts go to show that the time represented by the Lower Old Red Sandstone itself must have been enormous.

Now, three miles of rock removed since the commencement of the Old Red Sandstone period (which in all probability s an under-estimate) would give us 45 million years.

Again, going further back, we find the lapse of time represented by the Silurian period to be even more striking than that of the Old Red Sandstone. The uncomformities in the Silurian series indicate that many thousands of feet of these strata were denuded before overlying members of the same great formations were deposited. And again, this immense formation was formed in the ocean by the slow denudation of pre-existing Cambrian continents, just as these had been built up out of the ruins of the still prior Laurentian land. And even here we do not reach the end of the series, for the very Laurentians themselves resulted from the denudation not of the primary rocks of the globe, but of previously existing sedimentary and probably igneous rocks of which, perhaps, no recognisable portion now remains.

Few familiar with the facts of geology will consider it too much to assume that the time which had elapsed prior to the Old Red Sandstone was equal to the time which has elapsed since that period. But if we make this assumption, this will give us at least 90 million years as the age of the stratified rocks.

That the foregoing is not an over-estimate of the probable amount of rock removed by sub-aërial denudation during past geological ages will appear further evident from the following considerations:—The mountain ridges of our globe, in most cases, as is well known, have been formed by sub-aërial denudation: they have been carved out of the solid block. They stand two thousand, four thousand, or five thousand feet high, as the case may be, simply because two thousand, four thousand, or five thousand feet of rock have been denuded from the surrounding country. The mountains are high simply because the country has been lowered. But it must be observed that the height which the mountains reach above the surrounding country does not measure the full extent to which the country has been lowered by denudation, because the mountains themselves have also been lowered.

The height of the mountains represents merely the extent to which the country has been lowered. In the formation of a mountain by denudation, say 3000 feet in height, probably more than 6000 feet of strata may have been removed from the surrounding country. The very fact of a mountain standing above the surrounding country exposes it the more to denudation, and it is certainly not an exaggerated assumption to suppose that whilst the general surface of the country was being lowered 6000 feet by denudation, the mountain itself was at least lowered by 3000 feet.

The very common existence of mountains two or three thousand feet in height, formed by sub-aërial denudation, proves that at least one mile must have been worn off the general surface of the country. It does not, of course, follow that the general surface ever stood at an elevation of one mile above the sea-level, since denudation would take place as the land gradually rose. We know that the land was once under the sea, for it was there that it was formed. It is built up out of the materials resulting from the carving out into hill and dale, through countless ages, of a previously existing land, just as this latter had resulted from the destruction of a still older land, and so on in like manner back into the unknown past.

It has now been proved, by the foregoing very simple and obvious method, that the age of the earth must be far more than 20 or 30 million years. This method, it is true, does not enable us to determine with anything like accuracy the actual age of the globe, but it enables us to determine with absolute certainty that it must be far greater than 20 million years. We have not sufficient data to determine how many years have elapsed since life began on the globe, for we do not know the total amount of rock removed by denudation; but we have data perfectly sufficient to show that it began far more than twice 20 million years ago.

But if the present order of things has been existing for more than 20 million years, then the sun must have been illuminating our globe for that period, and, if so, then there must have been some other source than that of gravitation from which the sun derived its energy, for gravitation, as we have seen, could only have supplied the present rate of radiation for about one-half that period.

It is perfectly true, as has been stated, that the length of time that the sun could, by its radiation, have kept the earth in a state fit for animal and vegetable life, must have been limited by the store of energy in the form of hea which it possessed. But it does not follow as a necessary conse-

quence, as is generally supposed, that this store of energy must have been limited to the amount obtained from gravity in the condensation of the sun's mass. The utmost that any physicist is warranted in affirming is simply that it is impossible for him to *conceive* of any other source. His *inability*, however, to conceive of another source cannot be accepted as a proof that there *is* no other source. But the physical argument that the age of our earth must be limited by the amount of heat which could have been received from gravity is in reality based upon this assumption—that, because no other source can be conceived of, there is no other source.

It is perfectly obvious, then, that this mere negative evidence against the possibility of the age of our habitable globe being more than 20 or 30 million years is of no weight whatever when pitted against the positive evidence here advanced, that its age must be far greater.

Now, in proving that the antiquity of our habitable globe must be far greater than 20 or 30 million years, we prove that there must have been some other source in addition to gravity from which the sun derived his store of energy; *and this is the point which I have been endeavouring to reach by this somewhat lengthy discussion.*

Are we really under any necessity of assuming that the sun's heat was wholly, or even mainly, derived from the condensation of his mass by gravity? According to Helmholtz's theory of the origin of the sun's heat by condensation, it is assumed that the matter composing the sun, when it existed in space as a nebulous mass, was not originally possessed of temperature, but that the temperature was given to it as the mass became condensed under the force of gravitation. It is supposed that the heat given out was simply the heat of condensation. But it is quite conceivable that the nebulous mass might have been possessed of an original store of heat previous to condensation.

It is quite possible that the very reason why it existed in such a rarefied or gaseous condition was its excessive temperature, and that condensation only began to take place when the mass began to cool down. It seems far more probable that this should have been the case than that the mass existed in so rarefied a condition without temperature. For why should the particles have existed in this separate form when devoid of the repulsive energy of heat, seeing that, in virtue of gravitation, they had such a tendency to approach one another?

It will not do to begin with the assumption of a cold nebulous mass, for, the moment that the mass existed as

such, condensation—under the influence of the mutual attraction of its particles—would commence. We must therefore assume either that the mass was created at the moment condensation began, or that, prior to this moment, it existed under some other form. There are few, I think, who would be willing to adopt the former alternative. If we adopt the latter we must then ask the question, In what condition did this mass exist prior to the commencement of condensation? The answer to this question would naturally be that it existed in a condition of excessive temperature, the repulsive force of heat preventing the particles approaching one another. In short, the excessive temperature was the very cause of the nebulous condition.

But if the mass was originally in a heated condition, then in condensing it would have to part not only with the heat of condensation, but also with the heat which it originally possessed.

It is therefore evident that if we admit that the nebulous mass was in a state of incandescence prior to condensation, it will really be difficult to fix any limit either to the age of the sun or to the amount of heat which it may have originally possessed. The 20 million years' heat obtained by condensation may in such a case be but a small fraction of the total quantity possessed by the mass.

The question now arises—By what means could the nebulous mass have become incandescent? From what source could the heat have been obtained? The dynamical theory of heat affords, as was shown several years ago,* an easy answer to this question. The answer is that *the energy in the form of heat possessed by the mass may have been derived from Motion in Space*. Two bodies, each one-half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, would by their concussion generate in a *single moment* 50 million years' heat. For two bodies of that mass, moving with a velocity of 476 miles per second, would possess 4149×10^{33} foot-pounds of kinetic energy, and this converted into heat by the stoppage of their motion would give out an amount of heat which would cover the present rate of the sun's radiation for a period of 50 million years.

Why may not the sun have been composed of two such bodies? And why may not the original store of heat possessed by him have all been derived from the concussion of these two bodies? Two such bodies coming into collision with that velocity would be dissipated into vapour and converted into a nebulous mass by such an inconceivable amount of heat as would thus be generated; and when

* Phil. Mag. for May, 1868.

condensation on cooling took place, a spherical mass like that of the sun would result. It is perfectly true that two such bodies could never attain the required amount of velocity by their mutual gravitation towards each other. But there is no necessity whatever for supposing that their velocities were derived from their mutual attraction alone: they might have been approaching each other with the required velocity wholly independent of gravitation.

We know nothing whatever regarding the absolute motion of bodies in Space; and, beyond the limited sphere of our observation, we know nothing even of their relative motions. There may be bodies moving in relation to our system with inconceivable velocity. For anything that we know to the contrary, were one of these bodies to strike our earth the shock might be sufficient to generate an amount of heat that would dissipate the earth into vapour, though the striking body might not be heavier than a cannon-ball. There is, however, nothing very extraordinary in the velocity which we have found would be required to generate the 50 million years' heat in the case of the two supposed bodies. A comet having an orbit extending to the path of the planet Neptune, approaching so near the sun as to almost graze his surface in passing, would have a velocity of about 390 miles per second, which is within 86 miles of that required.

It must be borne in mind, however, that the 476 miles per second is the velocity at the moment of collision; but more than one-half of this would be derived from the mutual attraction of the two bodies in their approach to each other. Suppose, for simplicity of calculation, each body to be equal in volume to the sun, and of course one-half the density, the amount of velocity which they would acquire by their mutual attraction would be 274 miles per second. Consequently we have to assume an original or projected velocity of only 202 miles per second. And if the original velocity was 676 per second, the total amount of heat generated would suffice for 200 million years at the present rate of radiation.

On former occasions* I expressed it as my opinion that the total quantity of heat possessed by the sun could not probably exceed 100 million years' heat. But if we admit that the heat was derived from Motion in Space, there really does not seem any reason why it may not be double that amount.

It will be asked—Where did the two bodies get their motion? It may as well, however, be asked—Where did

* Phil. Mag., May, 1868. Climate and Time, chap. 21.

they get their existence? It is just as easy to conceive that they always existed in motion as to conceive that they always existed at rest. In fact, this is the only way in which energy can remain in a body without dissipation into Space. Under other forms a certain amount of the energy is constantly being transformed into heat which never can be re-transformed back again, but is dissipated into Space as radiant heat. But a body moving in void stellar space will, unless a collision takes place, retain its energy in the form of motion untransformed for ever.

It will perhaps be urged as an objection that we have no experience of bodies moving in space with velocities approaching to anything like 400 or 600 miles per second. A little consideration will, however, show that this is an objection which can hardly be admitted, as we are not in a position to be able to perceive bodies moving with such velocities. No body moving at the rate of 400 miles per second could remain as a member of our solar system. Beyond our system, the only bodies visible to us are the nebulæ and fixed stars, and they are visible because they are luminous. But the fixed stars are beyond doubt suns similar to our own; and if we assume that the energy in the form of heat and light possessed by our sun has been derived from Motion in Space, we are hardly warranted in denying that the light and heat possessed by the stars were derived from another source. It is true that the motion of the stars in relation to one another, or in relation to our system (and this is the only motion known to us), is but trifling in comparison to what we even witness in our solar system. But this is what we ought, *à priori*, to expect; for if their light and heat were derived from Motion in Space, like that of our sun, then, like the sun, they must have lost their motion. In fact, *they are suns, and visible because they have lost their motion*. Had not the masses of which these suns were composed lost their motion they would have been non-luminous, and of course totally invisible to us. In short, we only see in stellar space those bodies which, by coming into collision, have lost their motion, for it is the lost motion which renders them luminous and visible.*

* When the foregoing theory of the origin of the sun's heat was advanced, in 1868, I was not aware that a paper on the "Physical Constitution of the Sun and Stars" had been read before the Royal Society by Mr. G. Johnstone Stoney, in which he suggested that the heat possessed by the stars may have been derived from collisions with one another. "If two stars," he says, should be brought by their proper motion very close, one of three things would happen:—Either they would pass quite clear of one another, in which case they would recede to the same immense distance asunder from which they had come; or they would become so entangled with one another as to emerge

The formation of a sun by collision is an event that would not be likely to escape observation if it occurred within the limits of visibility in space. But such an event must be of very rare occurrence, or the number of stars visible would be far greater than it is. The number of stars registered down to the seventh magnitude, inclusive, is—according to Herschel—somewhere between 12,000 and 15,000, and this is all that can possibly be seen by the naked eye. Now, if we suppose each of them to shine like our sun for (say) 100 million years, then one formed in every 7000 or 8000 years would maintain the present number undiminished. But this is the number included in both hemispheres, so that the occurrence of an event of such unparalleled splendour and magnificence as the formation of a star or rather nebula—for this would be the form first assumed—is what can only be expected to be seen on our hemisphere once in about 15,000 years.

The absence of any historical record of such an event having ever occurred can therefore be no evidence whatever against the theory.

Note on Sir William Thomson's Arguments for the Age of the Earth.

Sir William Thomson has endeavoured to prove the recent age of the earth by three well-known arguments of a purely physical nature:—The first is based on the age of the sun's heat; the second, on the tidal retardation of the earth's rotation; and the third, on the secular cooling of the earth.

Argument from the Age of the Sun's Heat.—It will be obvious

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from the frightful conflagration which would ensue, as one star; or, thirdly, they would brush against one another, but not to the extent of preventing the stars from getting clear again." In the latter case he considers a double star is formed. Mr. Stoney's paper, though read in 1867, was not published till 1869.

Mr. Herbert Spencer, in his "First Principles" (pp. 532 to 535), has also directed attention to the fact that the stars distributed through space must tend, under the influence of gravity, to concentrate and become locally aggregated. Separate aggregations will be drawn towards one another, and ultimately coalesce. The result will be that the heat evolved by such collisions taking place under the enormous velocities acquired by gravity must have the effect of dissipating the matter of which they are composed into the gaseous state.

Both Mr. Stoney and Mr. Spencer consider the motions of the cosmic masses to be due wholly to gravity, but, as we have seen, gravity alone cannot account for the enormous amount of energy originally possessed by the sun.

that, if what has already been advanced in regard to the origin of the sun's heat be correct, it will follow that the argument for the recent age of the earth, based upon the assumption that the sun could have derived its store of heat only from the condensation of its mass, must be wholly abandoned, and that, in so far as this argument is concerned, there is no known limit to the amount of heat which the sun may have possessed, or to the time during which it may have illuminated the earth.

Argument from Tidal Retardation.—It is well known that, owing to tidal retardation, the rate of the earth's rotation is slowly diminishing; and it is therefore evident that if we go back for many millions of years, we reach a period when the earth must have been rotating much faster than now. Sir William's argument is,* that had the earth solidified several hundred millions of years ago, the flattening at the Poles and the bulging at the Equator would have been much greater than we find them to be. Therefore, because the earth is so little flattened, it must have been rotating, when it became solid, at very nearly the same rate as at present. And as the rate of rotation is becoming slower and slower, it cannot be so many millions of years back since solidification took place. A few years ago I ventured to point out† what appeared to be a very obvious objection to this argument, viz., that the influence of sub-aërial denudation in altering the form of the earth had been entirely overlooked. It has been proved, as we have seen, that the rocky surface of our globe is being lowered, on an average, by sub-aërial denudation, at the rate of about 1 foot in 6000 years. It follows as a consequence, from the loss of centrifugal force resulting from the retardation of the earth's rotation occasioned by the friction of the tidal wave, that the sea-level must be slowly sinking at the Equator and rising at the Poles. This, of course, tends to protect the polar regions and expose equatorial regions to sub-aërial denudation. Now it is perfectly obvious that unless the sea-level at the Equator has, in consequence of tidal retardation, been sinking during past ages at a greater rate than 1 foot in 6000 years, it is physically impossible the form of our globe could have been very much different from what it is at present, whatever may have been its form when it consolidated, because sub-aërial denudation would have lowered the Equator as rapidly as the sea sank. But in equatorial regions the rate of denudation is no doubt much greater than 1 foot in 6000 years, because there the

* Trans. Geol. Soc. of Glasgow, vol. iii., p. 1.

† Nature, August 21, 1872. Climate and Time, p. 335.

rainfall is greater than in the temperate regions. It has been shown that the rate at which a country is being lowered by subaërial denudation is mainly determined not so much by the character of its rocks as by the sediment-carrying power of its river systems. Consequently, other things being equal, the greater the rainfall the greater will be the rate of denudation. We know that the basin of the Ganges, for example, is being lowered by denudation at the rate of about 1 foot in 2300 years; and this is probably not very far from the average rate at which the equatorial regions are being denuded. It is therefore evident that sub-aërial denudation is lowering the Equator as rapidly as the sea-level is sinking from loss of rotation, and that consequently we cannot infer from the present form of our globe what was its form when it solidified. In as far as tidal retardation can show to the contrary, its form, when solidification took place, may have been as oblate as that of the planet Jupiter. There is another circumstance which must be taken into account. The lowering of the Equator, by the transference of materials from the Equator to the higher latitudes, must tend to increase the rate of rotation, or, more properly, it must tend to lessen the rate of tidal retardation.

The argument may be shown to be inconclusive from another consideration. The question as to whether the earth's axis of rotation could ever have changed to such an extent as to have affected the climate of the Poles is at present exciting a good deal of attention. The subject has recently been investigated with great care by Professor Haughton,* Mr. George Darwin,† the Rev. J. F. Twisden,‡ and others, and the general result arrived at may be expressed in the words of Mr. G. Darwin :—"If the earth be quite rigid no re-distribution of matter in new continents could ever have caused the deviation of the Pole from its present position to exceed the limit of about 3°."

Mr. Darwin has shown that, in order to produce a displacement of the Pole to the extent of only 1° 46′, an area equal to one-twentieth of the entire surface of the globe would have to be elevated to the height of two miles. The entire continent of Europe elevated two miles would not deflect the Pole much over half a degree. Assuming the mean elevation of the continents of Europe and Asia to be 1000 feet, Prof. Houghton calculates that their removal would displace the Pole only 199·4 miles.

* Proc. Roy. Soc., vol. xxvi., p. 51.
† Proc. Roy. Soc., vol. xxv., p. 328.
‡ Paper read before the Geological Society, February 21st, 1877.

It may now be admitted as settled that if the earth be perfectly rigid the climate of our globe could never possibly have been affected by any change in the axis of rotation. But it is maintained that if the earth can yield as a whole, so as to adapt its form to a new axis of rotation, the effects may be cumulative, and that a displacement of the Pole as much as 10° or 15° is possible.*

But then if the earth be able to adapt its form to a *change in the axis of rotation*, there is no reason why it may not be able to adapt its form to a *change in the rate of rotation*, and, if so, the flattening at the Poles and the bulging at the Equator would diminish as the rate of rotation diminished, even supposing there were no denudation going on.

Argument from the Secular Cooling of the Earth.—The earth, like the sun, is a body in the process of cooling, and it is evident that if we go back sufficiently far we shall reach a period when it was in a molten condition. Calculating by means of Fourier's mathematical theory of the conductivity of heat, Sir William Thomson has endeavoured to determine how many years must have elapsed since solidification of the earth's crust may have taken place. This argument is undoubtedly the most reliable of the three. Nevertheless, the data on the subject are yet very imperfect, so that no definite and trustworthy result can be arrived at by this means as to the actual age of the earth. In fact this is obvious from the very wide limits assigned by him within which solidification probably took place. "We must," quoting Sir William's own words on the subject, "allow very wide limits on such an estimate as I have attempted to make; but I think we may, with much probability, say that the consolidation cannot have taken place less than 20,000,000 years ago, or we should have more underground heat than

* A displacement of the Pole of less than 15° or 20° would be of very little service in accounting for the warm climate of Greenland during the Miocene and other periods. But a displacement to that extent, even supposing we admit the earth to be yielding, demands a condition of things which few geologists would be willing to grant. When it becomes generally recognised to what an enormous extent the temperature of the Arctic regions is dependent upon ocean currents, the difficulties in understanding how those regions have once enjoyed a temperate climate will disappear. Were the ice removed from Greenland that region would at present enjoy a warm summer, suitable for plant and animal life. It is the presence of ice rather than a positive deficiency of heat that makes Greenland so cold and barren (*see* Climate and Time, Chap. IV.). An increase in the quantity of heat conveyed by ocean-currents, merely sufficient to prevent the accumulation of ice, would completely transform the climate of the Arctic lands. And such an increase would take place during an Inter-glacial period when the eccentricity of the earth's orbit was at a high value and the winter solstice in perihelion.

we actually have,—nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of 'emergence' of the 'consistentur status' was probably within these dates."*

III. THE GLACIAL PERIOD IN THE SOUTHERN HEMISPHERE.

By Thomas Belt, F.G.S.

THE tablets on which the ice of the Glacial period left its record in the southern hemisphere are probably now mostly covered by the sea, and we cannot trace its progress and extent with the same facility and certainty as in the northern temperate regions. Yet notwithstanding this, and also that the land surfaces of the South that have been glaciated have not been studied to anything like the same extent as in Europe and North America, points of resemblance are apparent, and grounds exist for the belief that both hemispheres have passed through a somewhat similar glacial experience.

In our hemisphere, I have sought to show in former papers, there were two ways in which the ice spread. One was an accumulation on mountain-chains, and a radiation from them over the surrounding country. The other, and I think by far the most important, was the gradual advance of a ridge of ice down the bed of the North Atlantic, and probably also of the North Pacific, which blocked up the drainage of the continents as far as it extended, and caused enormous lakes of fresh or brackish water and immense destruction of life amongst the animals that were caught on the plains by the rising floods. The marks left by the Atlantic ice are seen in Europe, as far as the southern extremity of Ireland, and in America, to the south of New York, and beyond these points, its further progress can be traced by the evidence of the interruption of the drainage of the continents as far as the northern slopes of the Pyrenees on one side of the Atlantic, and to the coasts of

* Trans. Roy. Soc. of Edinburgh, vol. xxiii., p. 161.

Virginia on the other. The real markings on the rocks, however, have not been traced in Europe farther south than 51° N. lat., and in America than 40° N. lat., excepting those of the ice that proceeded from the mountain ranges.

In the other hemisphere, within the distance from the Southern Pole that the ice has been shown by actual markings on the rocks to have reached from the North Pole on the European coasts,—that is, to latitude 51° N.,—there are no large masses of land, excepting the extreme end of South America and the Antarctic continent. And if we take a corresponding circle in the south to the limit the ice has left its marks in America or to lat. 40°, we shall still only embrace Patagonia in South America and the Middle Island in New Zealand. Even if we take the furthest limit of the extension of the Atlantic ice, as shown by its interference with the drainage of the American continent, we only bring South America as far north as the Rio Plata, and New Zealand and Tasmania, with the southern end of Australia, within the area where we could expect to find any similar evidence on the supposition that in the Glacial period the ice extended everywhere as far from the Southern Pole as its extreme limit reached from the Northern.

The conditions are therefore very different in the two hemispheres. In the one, broad continents stretch from within the Arctic circle toward and up to the Equator; in the other, nearly the whole of the temperate zone is covered with water. If, then, there were much less evidence than there is of the glaciation of southern lands, we need not have been surprised; but of late there seems to have arisen an idea amongst some geologists that there is no evidence in the southern hemisphere of the occurrence of a Glacial period, and it may be useful if I bring together what has been described, and show how far the phenomena agree with those of the northern hemisphere.

Commencing in America, immediately south of the Equator, we have first to deal with the remarkable theory of Agassiz, that the great valley of the Amazon was once filled with ice flowing from the distant Andes, which left an enormous terminal moraine on the Atlantic coast. This moraine he supposed blocked up for a time the waters of the great valley, and caused the deposition of various stratified deposits covered by a peculiar drift clay that rarely contains transported boulders. I have examined this deposit from Pernambuco northwards through the provinces of Ceara and Maranham, as far as Para. In some parts it is composed of small angular fragments of rock, cemented together by an

ochraceous clay, and resembling the brecciæ at the base of the Permian rocks in Westmoreland. In others it is a ferruginous clay, containing few stones, but with a layer of quartz pebbles at its base. It is not confined to the valleys, but wraps over the hills like a mantle. Now and then it contains large boulders of granite, but I could never satisfy myself that these might not have been left during the decomposition and denudation of the rocks of the neighbourhood. It differs much from any glacial deposit I have seen, and I am sorry that I can neither suggest any theory to account for its origin nor agree with that of the illustrious Agassiz. To the latter there seems to me insuperable objections. There are no moraines in the valley of the Lower Amazon. The terminal one might have been, as Agassiz suggests, washed away by the waves of the Atlantic, but the great glacier ought to have left others to mark the various stages of its recession. No remnant of these has been found, and we cannot believe that a glacier that left a huge moraine stretching for hundreds of miles across the whole seaward front of the Amazon Valley should have shrunk back for more than 1000 miles without leaving any whatever to mark its retreating course. The Valley of the Amazon abounds with birds and beasts, many of which are found nowhere else. Peculiar species of fishes swarm in the river and its tributaries. If the great valley was filled with ice, where did these find a refuge? To Agassiz this did not present any difficulty, as he believed that the present inhabitants of the world had been created since the Glacial period; but to those who hold the opinion that they are descended from pre-glacial ancestors, and that since the Glacial period there has not been much variation, the peculiar genera and species of the fauna of the Amazon Valley present serious objections to the theory.

Had Agassiz found in the Valley of the Amazon what he considered moraines, I should have had much difficulty in believing that he was mistaken, for no man had more experience of ice-action, present and past. Before him, Charpentier and others had worked out the conclusion that the glaciers of the Alps had in former days stretched far beyond their present limits, but they referred that extension to an elevation of the mountains, and not to a change of climate that affected all the northern parts of the continent. Agassiz accepted the theory, at first, that the upheaval of the Alps must, in some way or other, have been connected with the phenomena, but further study led him to abandon it, and conclude that the climatic conditions could not have

been local, but must have been cosmic. "When," he says, "Switzerland was bridged across from range to range, by a mass of ice stretching southward into Lombardy and Tuscany, northward into France and Burgundy, the rest of Europe could not have remained unaffected by the causes which induced this state of things."* Agassiz was thus the founder of the theory of a "Glacial Period," and in Great Britain, in Scandinavia, and in North America, evidence was soon found, and has been ever since accumulating, attesting the truth of his grand generalisation.

He was not likely to be mistaken as to what constituted a moraine, but he found none in the great valley, and he says himself that he had not here the positive evidence that had guided him in his previous glacial investigations.† Not so, however, with regard to his discovery of the marks of glaciation on the mountains of Ceara. Here, only about three degrees south of the Equator, he found undoubted moraines blocking up the valleys, and the evidence of glacial action was to him as clear as in the valleys of Switzerland, of Scotland, and of the Northern States of America.‡ This is the nearest point to the Equator at which glacial moraines have been found. On the other side of the line I found huge moraines in the northern part of Nicaragua, near the boundary between it and Honduras, in lat. 13° 47′ N.‖ This is the farthest south that glacial action has been traced in the northern hemisphere. Prof. W. M. Gabb has informed me that in his geological researches in the mountains of Costa Rica he found no evidence of glaciation. Between the moraines of northern Nicaragua and those of the mountains of Ceara there is an area comprising about 17° of latitude, and including most of Nicaragua, the whole of Costa Rica, of Columbia, of Equador, and of the great valley of the Amazons, in which no certain signs of glaciation have been seen. This wide region includes several large zoological sub-provinces, characterised by highly peculiar tropical genera, and a wealth of species not met with elsewhere on the continents to the north and south. Within this area are large groups of insects the extreme forms of which are linked together by a series of gradations, and every district has its representative forms of types that run through the whole. But as we travel south from the unglaciated districts lying between lat. 14° N. and lat. 3° S. the

* Geological Sketches, Second Series, p. 29.
† *Ibid.*, p. 207.
‡ A Journey in Brazil, by Mrs. Agassiz, p. 456.
‖ Naturalist in Nicaragua, p. 260.

species become more and more separated; the genera comprise species as far removed from each other as before, but these are not connected by the same intermediate forms. These gaps become wider and wider as we get farther south, until at the extreme end of the continent, where the rocks are glaciated from ocean to ocean, isolation, and not affinity, is the characteristic of the fauna, which is made up of waifs and strays that appear rather to have struggled in from the outside, upon a country that had been depopulated, than to have been developed within it.

In the glaciers of Ceara and northern Nicaragua we find the first parallel between the glaciation of the two hemispheres; and it is to be noted here, as it may be elsewhere, that there appears to have been more ice heaped up on the southern half of the world than on the northern, the much nearer approach of the glaciers to the Equator in Brazil than in Central America being the first evidence of it. This was probably due, not to greater cold, but to greater precipitation, proportional to the vast evaporating area of the Southern Ocean.

Travelling southward, we find that Mr. David Forbes noticed in Bolivia great accumulations of *detritus* with grooved stones and deeply-furrowed rocks, resembling those that he was familiar with in Norway.* And on the opposite side of the continent, near Rio Janeiro, Prof. Hartt has described moraines left by glaciers that formerly came down from the mountains of Tijuca.†

Still farther south the evidences of glacial action increase. For the description of the phenomena of La Plata, Patagonia, and Chile we must still turn to the observations of Darwin made more than forty years ago, which, when we consider that the glacial theory was in its infancy, evince the same rare powers of acute observation and philosophical generalisation that have since made his name so famous.

Ascending the River Santa Cruz, Darwin found, at a distance of about 100 miles from the Atlantic and at a height of about 1400 feet above the level of the sea, a great abundance of large angular boulders that had been transported from the Cordillera, the nearest slope of which was still about 60 miles distant. On both sides of the continent from lat. 41° S. to the southern extremity of it, he considers there is the clearest evidence of former glacial action in numerous immense boulders transported far from their parent source.‡

* Darwin, Origin of Species, Sixth Edition, p. 335.
† Geology and Physical Geography of Brazil, p. 26.
‡ Origin of Species, Sixth Edition, p. 335.

He describes Terra del Fuego as largely covered with till and boulder clay perfectly unstratified, and in which he looked in vain for marine remains.

On the western coast the ice from the Chilian Cordillera appears to have flowed across the Straits to the island of Chiloe, as Darwin found it covered with large boulders of granite and syenite that had come from the mainland. On one of the Chonos Islands he found a quantity of comminuted marine shells in the drift, and in Chiloe two or three fragments of a *Cytherea*.* We may compare these facts with those in the northern hemisphere, where the drift in the southern shores of the Baltic, brought by land ice from Scandinavia, is found—as in the vicinity of Bromberg—to contain broken and worn sea-shells that have probably been picked up in the passage of the ice across the ocean-bed.

Bearing just the same relation to the Glacial period in South America as the loess of the Rhine and the Danube does in Europe, and still more resembling the diluvial clay of the South of Russia, the Pampean mud is spread out in Rio Plata, Banda Oriental, and Entre Rios. It covers an immense tract of country. Darwin passed continuously over it from the Rio Colorado to St. Fé Bajada, a distance of 500 geographical miles, whilst M. d'Orbigny traced it for 250 miles farther north. From east to west, in the latitude of the Plata, it extends for at least 300 miles.

It is a reddish, slightly indurated, argillaceous earth, sometimes more than 100 feet thick, with lines of calcareous concretions, and occasionally changing into a compact marly rock. Marine shells are found scattered over the surface of this deposit at some places; but Darwin notices, as a remarkable fact, their absence throughout the deposit, excepting in the uppermost layers near Buenos Ayres. Even microscopical organisms appear to be very rare, and only found in the lower part of the deposit. Thus, in some of the red mud scraped from the tooth of a Mastodon found at the bottom of the Pampean mud at Gorodona, Prof. Ehrenberg found seven species of *Polygastrica* and thirteen species of *Phytolitharia*. Of these nearly all are of fresh-water origin, only three being marine. At Monte Hermoso, in Bahia Blanca, the lower part of the Pampean mud contains a similar assemblage of microscopical organisms. Prof. Ehrenberg considers that they must have lived in brackish water.

The most remarkable facts respecting the Pampean

* Trans. Geol. Soc., vol. vi., p. 426.

formation are its vast extent and the great number of the remains of large extinct mammals imbedded in it. Mr. Darwin says that he is firmly convinced that it would be impossible to cut a deep trench in any line across the Pampas without meeting with the remains of some quadruped. Wherever sections are exposed skeletons or detached bones are found, so that "the whole area of the Pampas is one wide sepulchre of these extinct gigantic quadrupeds."* At Punta Alta, within an area of about 200 yards square, were found the remains of no less than nine species of gigantic extinct mammals, including three Megatheriums, a Megalonyx, a Mylodon, and an extinct horse. Some of the skeletons were nearly perfect, besides which there were many detached bones. On the banks of the Parana two entire skeletons of the Mastodon were found near the base of the Pampean mud.

The Pampas extend southwards to the Rio Colorado. There, beds of gravel begin to take the place of the Pampean mud. These are at first thin, and composed of small pebbles. Farther south the gravel-beds are composed of coarser pebbles, with sometimes large boulders. This deposit of shingle extends for 800 miles up to the Straits of Magellan. In some parts the plains of gravel rise in step-formed terraces to a height of 1200 feet above the sea. Marine shells are scattered over the surface of the plains, but are not found in the beds of gravel, which, like the Pampean mud, contain in some parts numerous remains of the great extinct quadrupeds.

Darwin mentions that the Mammalian remains have not anywhere been found in existing marshes or peat-beds: all are entombed in the body of the deposit of which the plains are composed.

In the Pampas, in the caves of Brazil, and in deposits of similar age in other parts of South America, more than one hundred extinct species of quadrupeds, many of great size, have been found. There are representatives of the sloths and armadillos as large as existing elephants. There are also representatives of genera that do not now exist in South America, but still live in other parts of the world—such as leopards and antelopes. The remains of the horse are abundant, though when the Spaniards discovered America it was extinct there.

Mr. Wallace, in his great work on the distribution of

* Darwin, Naturalist's Voyage, p. 155. The whole of my information respecting the Pampas is derived from this work and from The Geological Observations on South America by the same author.

animals, has given an excellent summary of the facts known respecting these lost animals, and has placed the question of their extinction before us with clear-cut distinctness.* He shows that in very recent geological times a great change has taken place in the fauna not only of South America, but of North America and Europe, and that this change is unprecedented in older geological periods. We now live, he says, "in a zoologically impoverished world, from which all the hugest and fiercest and strangest forms have disappeared." He urges, with luminous force, that there must have been some physical cause for this great change, which must have acted at the same time over large portions of the earth's surface. Such a cause he considers is to be found in that great era, the glacial period.

There is much to be said in favour of Mr. Wallace's conclusion. The Pampean mud occupies the same relation to recent deposits as other clays whose glacial age has been satisfactorily determined. Both it and the gravel-beds of Patagonia, in many places, lie directly above marine deposits of undoubted late Pliocene age, as they contain shells of *Ostrea Patagonica*. They hold, therefore, the same position between the tertiary and recent deposits that glacial beds occupy in other countries. The Patagonian deposits also contain large boulders that have been transported from the Cordillera.

The Pampean mud has been compared with the loess of the Rhine—a similar fine slightly-indurated clay, which contains the bones of the extinct mammoth and woolly rhinoceros. It appears to be still more like the beds of diluvial clay that form the wide-spread steppes of Southern Russia. This clay is of undoubted Glacial age, and when traced northward contains large blocks of stone derived from rocks hundreds of miles to the north, just as the Pampean mud when traced southward is replaced by gravel-beds with boulders from the Cordillera. In the January number of this Journal I have advocated the theory that the diluvial clays of Northern Europe and the loess of the Rhine and the Danube were spread out in a great lake, when the bed of the Atlantic was occupied by ice that stopped the drainage of Europe as far as it extended. In eastern North America there is evidence of a similar interruption to the drainage of the continent to at least as far as North Virginia.

It is a remarkable fact that to about as far from the South Pole in South America as from the North Pole in North

* Geographical Distribution of Animals, vol. i., p. 143.

America there is the same evidence of the land having been covered with water which was not that of the sea; as the deposits left by it do not contain marine remains. If we may suppose that a similar mass of ice accumulated around, and spread from the Antarctic as from the Arctic regions, and that ultimately a prolongation or arm of that icy mass reached the eastern coast of South America, so as to dam back the streams as far as the Rio Plata, we shall have, I believe, a complete explanation of the deposition of the Pampean mud and the Patagonian gravels, and of the destruction of the great mammals and the entombment of their remains.

The present physical geography of the world and the existing distribution of its inhabitants were immediately preceded by a wide-spread entombment of land animals, unparalleled elsewhere in the geological record. In lake basins and estuaries of Eocene and Miocene age there have been preserved abundant remains of land animals, more especially in connection with the movements of elevation of certain mountain chains and the volcanic phenomena that accompanied them; but, compared with that which we are discussing, these entombments are insignificant and partial. The destruction of life that took place in the Glacial period was continental, if not world-wide, in its extent. The deposits in which the remains are found were not formed in old lake basins; they fringe, and in some cases nearly overspread, the continents, or run in great arms up the larger valleys. Here and there in America, Europe, and Asia, there are isolated deposits containing the remains of tertiary mammals, but the bones of the glacial mammals are spread over all the northern parts of the northern continents, and in South America are of equal extent.

The blockage of the coasts by ice flowing down the ocean beds seems just such an event as would bring about the destruction of life and the entombment of the remains of which we have so much evidence. I shall have to return to the consideration of the possibility of such a mass of ice having advanced northwards from the Antarctic circle when we have examined the evidences of glaciation in New Zealand; but let us assume now for a moment that such an accumulation did take place, and that it flowed down the eastern coast of South America; then, as it blocked up the drainage of each great valley, progressively from the south, northward, the waters pounded back would rise and overwhelm the animals living on the plains. The nature of the deposits favours this supposition. The lowest parts, or

those first formed, contain some evidence of brackish water conditions such as might have been produced by the embracement of the salt water of bays within the area blocked up by the ice; but the upper parts contain nothing but the remains of land animals, whilst the proof of the presence of water is complete in the rounded and stratified shingle forming the Patagonian plains.

Mr. Wallace has shown that the cave fauna in Brazil differs from that of the Pampas in containing a larger proportion of existing genera, and is inclined to believe that this may imply that the Pampean mud is a little older than the cave deposits.* It may, however, be due, I think, to the destruction of life having been more complete in Buenos Ayres and Patagonia than in Brazil, and that, in pre-glacial times, as now, the former countries formed a part of a distinct zoological sub-region. That the destruction of life in Brazil should have been less complete than farther south is likely—not only because we have no proof that the great flood extended so far north, but because there was a large extent of country, neither covered with ice nor overwhelmed with water, where many of the species might be preserved. In Patagonia and Buenos Ayres the lower country was covered with water, the upper mostly with glaciers descending from the mountains, so that the retreat of the animals from the flood was in a great measure cut off. The peculiar species that have been preserved in the Chilian sub-region, which includes Patagonia and Buenos Ayres, are principally alpine forms, such as the Chinchillas, the Alpacas, and the Viscachas, which we may suppose found a refuge during the great flood in some high lands not covered with glacier ice. Others may have retreated northward up the western side of the Andes, and returned southward after the greatest severity of the Glacial period had passed away. The remains of an extinct llama are found on the high plains of Mexico, so that the genus had undoubtedly a former greater extension northwards. The principal extirpation of life appears to have been amongst the bulky species, on whom the changes of environment would press most heavily, and which were confined to low-lying districts. Thus the Mastodon, whose remains are found at great heights in the Andes, appears to have escaped the complete destruction that befel its bulky companions at the time of the greatest extension of the ice, as I saw remains of it in Mexico that from their fresh appearance were probably as recent as those

* Geographical Distribution of Animals, vol. i., p. 146.

of the species that is found in post-glacial deposits in the Northern States.

Whilst the eastern coasts of Patagonia are fringed with plains of gravel, the western are indented with deep fiords like those of Norway, and, as Dana and Ramsay have shown, these fiords are evidence of great glaciation, and have probably been excavated by ice-action.

Leaving South America and passing to the continent of Africa, we find but little evidence of the Glacial period, as its southern extremity only reaches to about lat. 35° S. Mr. G. W. Stow has, however, shown that the southern ranges of mountains undoubtedly bore glaciers. Thus the Katberg range is in many parts rounded and smoothed, as if by the passage of ice, and on its northern slopes there are great mounds and ridges of unstratified clay packed with angular boulders of every size, from small gravel and pieces of a few pounds weight to masses of several tons.* The slopes of the Stormberg are similarly glaciated, and there are immense accumulations of morainic matter in all the valleys. Both lateral and terminal moraines have been observed in these mountains. In British Kaffraria, near Greytown, the ice would appear to have reached nearly to the present sea-level. Mr. Stow comes to the conclusion that the rounding-off of the hills in the interior, the numerous dome-shaped rocks, the enormous erratic boulders in positions where water could not have carried them, the frequency of unstratified clays, clays with imbedded angular boulders, drift and lofty mounds of boulders, and the large tracts of country thickly spread over with unstratified clays and superimposed fragments of rock,—all indicate the conditions that in other countries characterise the Glacial period.†

The Rev. W. B. Clarke has informed Mr. Darwin of facts from which it appears that there are traces of former glacial action on the mountains of the south-eastern corner of Australia, but I do not know the particulars.‡ I have not seen any notice of glacial phenomena in Tasmania, but it is extremely probable that they will be found. In New Zealand the evidences of former glaciation are clear and unmistakable, and they have been described by many able observers. The large glaciers that still exist in the mountains of the South Island bear about the same relation to the

* Quart. Journ. Geol. Soc., vol. xxvii., p. 539.
† *Ibid.*, p. 544.
‡ Darwin, Origin of Species, Sixth Edition, p. 335.

ancient ones as those of the European Alps do to the much larger ones of the Glacial period. The western sea-board is penetrated by long sounds or fiords, many of which are more than 1000 feet deep. On the eastern side of the range the fiords are replaced by arms of large and deep fresh-water lakes; and so far do the marine fiords on one side, and the arms of the fresh-water lakes on the other, cut into the mountain chain that in many places they reach to within 10 miles of each other.* The New Zealand geologists appear to be unanimous in ascribing the formation of the sounds and deep lakes to the action of ice. The former great extension of the glaciers is marked by immense morainic accumulations and transported boulders. These deposits have been ably described by Dr. Haast. For more than 90 miles south of the River Mikonuhi, on the western coast, all the lower country is covered by mounds and sheets composed of unstratified clays packed with boulders from the mountain chain. The moraines form low hills bounding the sea, the waves of which have cut them into cliffs. Along the whole of this shore glaciers must have descended from the New Zealand Alps down to, and probably beyond the present sea-level. Some of the imbedded blocks exposed in the sections on the sea-coast are of immense size, often larger than the celebrated Pierre-a-bot, in the Jura. These blocks have principally been brought from the very centre of the chain, and the distribution of the different rock formations in the drift and alluvial beds has led Dr. Haast to the conclusion that the configuration of the mountain chain was similar to what it is now at the time of the great extension of the glaciers.†

On the eastern side of the range the old glaciers did not reach to so low a level as on the western side, but terminated many miles from the sea-coast, which is bounded by great plains of stratified drift. In the northern part of the South Island Mr. Locke Travers has shown that many of the lakes are enclosed by huge moraines,‡ and in the North Island there is also evidence of intense glacial action.

The southern and eastern coasts of the South Island are bordered by great plains, composed of gravel and rounded boulders, which sometimes overlie beds of silt containing bones of the extinct moas. In the south part of the island the Southland Plains extend for nearly 40 miles along the

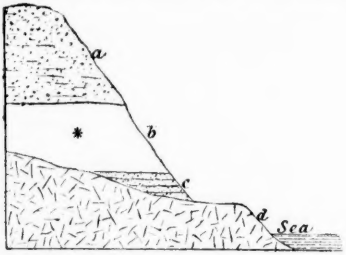
* Hutton and Ulrich, Report on the Geology and Gold-Fields of Otago. 1875.

† Quart. Journ. Geol. Soc., vol. xxiii., p. 350.

‡ *Ibid.*, vol. xxii., p. 254.

coast, and back from it for about 26 miles, besides sending arms up the valleys of the Mataura, Oreti, and Jacob Rivers. This plain is composed of shingle, sand, and clay, with some seams of lignite near the base of the deposits. It wraps around the Moonlight Range and Hokonui Hills, which stand up like islands out of it. The beds of which it is composed contain the bones of the great extinct apterous birds. Marine remains are not found, but Captain Hutton ascribes its formation to the action of the sea when the land stood at a lower level, as he finds it impossible to believe "that the alluvia of the Mataura, the Oreti, and the Jacob Rivers should all join one another with gradual slopes behind the seaward range of hills without the intervention of some uniform widely-acting cause, such as the sea."*

SECTION N. SIDE OF OAMARU (CAPTAIN HUTTON).



a. Gravels of the Plains.
b. Silt with Moa Bones at *.
c. Gravels with twenty-two species of Marine Shells, all but two still living on the New Zealand coasts.
d. Basalt.

These plains rise gradually from the sea-coast until they attain a height of about 600 feet next the mountains. On the coast they overlie marine deposits of late Pliocene age. The above section, from the work already quoted, shows the relation of the beds on the north side of Oamaru Cape.

The silt formation extends inland for a considerable distance, and is extensively developed at Hampden. Captain Hutton notes the great analogy it presents to the Pampean formation of South America. The remains of the extinct birds are distributed, like those of the extinct beasts at Buenos Ayres, throughout the deposit. The above section might indeed be almost exactly paralleled from South America, more especially in Patagonia, where the gravel-beds

* Geology and Gold-Fields of Otago, p. 79.

containing bones of extinct mammals overlie marine strata of late Pliocene age.

On the east coast, great plains, separated by rocky promontories, border the sea. The well-known Canterbury Plains are the greatest of these outspreads of gravel: they have been well described by Dr. Haast, and the question of their origin has been a fruitful source of controversy; they are 112 miles in length, and run back from the coast to the base of the mountains in the interior; gradually rising until they attain an elevation of about 1500 feet above the sea, according to Dr. Hochstetter.* These plains are composed of boulders, gravels, sand, and clay, and in some parts the deposits are upwards of 200 feet thick. Throughout the whole formation no marine remains have been found. At Banks's Peninsula the gravel-beds are replaced by deposits of silt that cover the volcanic hills to a height of about 800 feet above the sea. This fine loam must, from its description, closely resemble the loess of the Rhine and Danube, and it contains the bones of the great wingless birds and land-shells, but nowhere has the least trace of marine life been found in it.†

We have thus in New Zealand a repetition of the phenomena observed in Patagonia—great plains of gravel and silt on the eastern, and deep sounds or fiords on the western, coasts. In both countries the deposits of the plains entomb the remains of large extinct animals, and their geological age is fixed by both overlying marine beds of late Pliocene age. They are the representatives in position of the glacial beds of the northern hemisphere. Considering the very different distribution of land and water in the two hemispheres the resemblance of their glacial phenomena is most remarkable. Yet still more remarkable is it that most of the able naturalists of New Zealand not only deny that there is proof of a Glacial epoch in their country, but even are inclined to refuse us one elsewhere on the evidence they find there. I do not gather very clearly Dr. Haast's opinion on this question, and as a few years ago he was certainly in favour of New Zealand having been covered with an ice-sheet like Greenland, he may perhaps be excepted; but the remainder of the geologists, headed by Dr. Hector and Capt. Hutton, contend that the glaciation of New Zealand had no connection with a general glacial period, and that the great accumulation of ice was owing to the mountains

* New Zealand, English Edition, p. 508.

† Dr. Haast, Trans. New Zeal. Inst., vol. vi., p. 423.

having then been higher. Dr. Hector, who first suggested this explanation, candidly admits that the shore deposits do not support it, and considers that there must have been unequal movements of elevation, and that the former greater weight of the mountains was due to their greater bulk, which has since been reduced by denudation.

Dr. Hector, so far as I can make out from his writings, only seems to offer this as an alternative to accepting the occurrence of a general Glacial epoch, and we must turn to the works of Captain Hutton for the reasons that have weighed with the New Zealand geologists in rejecting the theory of a Glacial period, which, since Agassiz first propounded it for the northern hemisphere, has been adopted by nearly all our leading geologists. In the first place Captain Hutton thinks that the date of the last great glacier period of New Zealand, must be placed much further back than that of the northern hemisphere, because, since it occurred, some of the old lake basins have been filled up with deposits brought down by the streams; the surfaces of the rocks that were formerly covered with ice have been weathered to a depth of ten or twelve feet; old river courses have been filled with gravels, and the streams have cut new channels to great depths, so that in many places the drainage system of the country has been altered.* But in reality these facts should rather have been advanced as evidence of the analogy of the glacial phenomena of the two hemispheres instead of as a distinction between them. We have but to appeal to the erosion of the deep gorge of the Niagara, for at least a distance of three miles since the glaciation of Canada, to the reversal of the drainage of the great lakes from the Mississippi into the St. Lawrence, to the buried old river channels of the north of England and of Scotland, and to the numerous filled-up lake basins of Europe and America, to show that it is the resemblances and not the differences that strike us with greatest force.

Captain Hutton has not, however, restricted his argument to the physical side of the question, but has sought to strengthen it by appealing to the evidence of the organic remains. It is admitted that the Glacial deposits contain no marine organism, but, as we have already seen, they overlie beds of gravel and sand, with sea shells of late Pliocene age, as shown in the Oamaru section. These late Pliocene deposits are largely developed at Wanganui in Cook's Strait, and it is from a study of the present and past range of the species of the marine mollusks in these and similar beds,

* Geology and Gold Fields of Otago, p. 94.

that Captain Hutton's ingenious argument is founded. He has given a list of a great number of mollusks found in these beds that do not now live so far south, whilst only a few occur that do not now live so far north.* This, he thinks, proves that there was no reduction in the general temperature in times immediately preceding the glaciation of the country. We do not know, however, the age of these pre-glacial deposits, and they may be as far back in time as our coralline crag, and the value of the evidence has been much lessened by some facts described by Mr. C. W. Purnell. He states that at Wanganui there are three distinct fossiliferous strata, separated by thick beds of volcanic mud, and that the shells of these different horizons are mixed together in Captain Hutton's lists.† If this is so, it may yet appear, when the fauna of each zone is more critically studied, that the same evidence of a gradual refrigeration of the climate in the Pliocene epoch exists in New Zealand as in Europe.

Captain Hutton further argues that as many of the shells that extend back to the Miocene epoch, and still exist on the coast of New Zealand, are littoral species, and are not found elsewhere, we should have to suppose, if the extension of the glaciers was due to the change in the climate, that during the cold period they crossed the deep sea to Australia or Polynesia, and that, on the return of a warmer climate, they all came back again to New Zealand without leaving any behind on the coasts they had retired to during the Glacial period. He concludes, therefore, that since the Miocene period there can have been no reduction of temperature sufficient to account for the former extension of the glaciers, and that we must necessarily look to elevation of the land as the main cause. He admits, however, that it is possible that the two may have been combined, but considers that at present the evidence seems to be in favour of there never having been a Glacial epoch in New Zealand, and consequently none in the southern hemisphere.‡

Now, if the theories I have advocated respecting the Glacial period are correct, the two conditions of the glaciation of the land, and at the same time its relatively greater elevation above the level of the ocean, must have existed together. For such an amount of ice as is necessary could not be piled up around and outside the Arctic and Antarctic circles at the same time without abstracting so much water

* Trans. New Zeal. Inst., vol. viii., p. 383. Captain Hutton has also published an abstract of his views in the Geological Magazine, 1875, p. 580.

† Trans. New Zeal. Inst., vol. vii., p. 453.

‡ Ibid., vol. viii., p. 387.

from the ocean as to lower its level to the extent, I believe, of about 2000 feet; and this lowering of the ocean would give a coast line along which the littoral mollusks might retreat northwards during the Glacial period. In the line of soundings made between Australia and New Zealand by the officers of the *Challenger*, it was found that from the Australian shore the water deepened gradually until a depth was attained of 2600 fathoms, at about one-third the distance sccross. Nearer New Zealand the water shoaled suddenly, and at a distance of about 280 miles from its shore, a depth of only 275 fathoms, or 1650 feet, was found with a rocky bottom. These soundings were taken in one line only, and there can be scarcely a doubt that a lowering of the sea level 2000 feet would add more than 300 miles to the extension of New Zealand northward, and afford the necessary refuge for its fauna and flora during the Glacial period.

To prove the necessity of lands to the northward, now submerged, having then been above the sea, it was not necessary to appeal to the evidence of the marine shells, which is so far doubtful, as it is known that many shallow water species, under other circumstances, live at a great depth; as that of the inhabitants of the land was equally available and more certain. For the bones of the great extinct birds are not confined to the drift beds, but are found in old kitchen middens, ovens, encampments, and other places on the surface of the glacial beds, proving, that after the cold period had passed away, they returned again. It is certain that they must have been expelled from the existing area of the South Island, when glaciers came down beyond the present coast lines on the west side, and the great plains of drift were being formed under water on the east. The marine mollusks might have passed across some channels of the sea, but the apterous birds required a continuous land passage during their retreat, and the withdrawal of the waters of the ocean would have afforded this, not only to the North Island, but to the great plateau now submerged, whose existence has been indicated by the few soundings of the *Challenger* expedition.

We may agree with the New Zealand geologists that their country stood at a higher level above the ocean than it now does when it was glaciated; but instead of looking upon this as evidence in favour of local causes having led to the accumulation of the ice, it is only another link in the evidence to prove that the level of the ocean was lowered, and that the glaciation of the southern hemisphere took place at

the same time as that of the northern, as it occurred when the land stood relatively to the sea higher than it now does. There is the same objection to its being movements of the land that effected this, as there is to the theory of similar land movements in Great Britain, for the New Zealand geologists would send their country 3000 feet up into the air to allow the glaciers to descend to the present sea-level, and lower it 2000 feet to permit the gravels of the plains to be spread out, making a total movement of more than 5000 feet; yet after these enormous imaginary oscillations, we find everywhere, as we do in England, the pre-glacial shell beds with their littoral species only within a a few feet of the present shore line.

Let us now turn to the consideration of the question of the origin of the great plains of drift and silt that border the eastern and southern coasts. Dr. Haast, in his able essay on the structure and origin of the Canterbury Plains, considered that the gravels were the exuviæ from the great glaciers of the interior that had been spread out by the floods produced whilst it was melting. Some serious objections have been urged against this view. The sheets of gravel wrap around the hills, and are spread right across the water-sheds between different river systems. They are nearly level for scores, or even hundreds of miles. Ranges of hills are isolated so that they rise up from the plains like islands out of the sea. The waters, necessary to overflow such an extent of country, would be raging torrents, which, instead of depositing sediments, would sweep everything before them into the ocean; and would be rather agents of destruction and denudation than of deposition above the sea-level. There are beds of silt up to 800 feet above the sea-level at Banks's Peninsula, and for the deposition of these beds we require the presence of tranquil waters, and not torrential floods. It may also be remarked that it is not probable that, even on the hottest day, sufficient ice could be melted to produce the quantity of water required to submerge the country next the sea shore beneath a flood several hundred feet deep. The very statement of the enormous quantity of water required seems to condemn the theory when we remember that every day the water produced would be carried off, if near the sea there was nothing to stop its outflow and dam it back.

These considerations have led the generality of New Zealand geologists to adopt the theory that the great seaward plains are of marine formation, and that the gravels, sands, and loams were spread out when the land stood much

lower than now. The theory is the same as has been proposed to account for the similar outspreads of sheets of gravel and drift in other parts of the world in the Glacial period, and the same objection is to be made to it—the absence of marine remains. In the case of New Zealand, the drift has not been found to contain even the broken and worn sea shells that in Europe occur in some cases where ice had advanced on the land from the sea, bringing some of the productions of the latter with it.

If the theory is correct, the land must have sunk nearly 2000 feet, to allow of the formation of the Canterbury Plains. As it was slowly sinking and rising again, every portion of the area submerged would become a sea beach, and pass through the different stages of shallow and deep water; that at the present sea-level to a depth of about 2000 feet. As the land emerged, these stages would again be passed through. Along these successive coast-lines there would be bays and promontories, estuaries and sand banks, rocky cliffs and pebbly beaches. We have evidence, as Captain Hutton has shown to us, that the mollusks never left the shores. What then has become of their remains? We may grant that there must have been much destruction of the old sea beds during the rise of the land, but there ought to be some exceptions from that devastation, and not total and supreme annihilation. The beds themselves have not been destroyed; there are sheets of gravels, sands, and clays, lying as they are supposed to have been spread out during the submergence, but they contain no marine organism. Contrast these with the pre-glacial beds in the same districts, as, for instance, the clays and sands on the coast at Wanganui and Oamaru. These teem with sea shells, yet the unconsolidated sands and gravels of which they consist have passed through all the vicissitudes of the Glacial period. In Europe it is the same; the pre-glacial marine beds at Cromer and elsewhere, often of loose sand, are full of marine shells. We have also in Europe the post-glacial raised beaches, as in Scotland, up to 50 feet, and in Norway up to 600 feet above the sea-level, and these are often so crowded with marine shells, that in Norway they have been worked for many years for burning into lime.

We cannot believe that no life existed in these seas, for we know that it abounds both in the Arctic and Antarctic regions up to the foot of the great ice-sheet. Dr. Hooker states that along the shores of the Victoria Barrier the soundings were invariably charged with diatomaceous remains, that the water and the ice of the South

Polar ocean abound with them, and that they occur in such myriads that where washed on the bergs or pack-ice by the sea they stain it a pale ochreous colour. These remains were detected along every ice-bound shore, and in the depths of the adjoining ocean between 80 and 400 fathoms.* Nor is life in the polar regions restricted to these minute organisms, for recent Arctic explorations have proved that mollusks and crustaceans swarm at the very foot of the Greenland glaciers, and even to the north of Siberia where the water is freshened by the great floods, poured into the Arctic Ocean by the Obi and the Yenisei.

In New Zealand, Mr. Crawfurd has recognised the significance of the absence of marine life in the deposits forming the plains, and has suggested that they have been spread out in great fresh-water lakes. To account for the origin of these lakes he supposes that there was formerly a barrier of land running down the south-eastern coast, which has since entirely disappeared.† Agreeing as I do with Mr. Crawfurd, that the facts he has brought forward show that these deposits are not marine, it is difficult to believe in the possibility of a ridge of land having been elevated, since Pliocene times, along the whole coast of New Zealand, to a height of 2000 feet, and utterly destroyed since, so that not a vestige of it remains.

It will have been premised that I attribute the origin of these deposits, as I have those of similar ones in South America, to the advance of the Antarctic ice upon the coast so as to block up the drainage of the land. The great feature of the Glacial period is, of course, the enormous development and accretion of ice, and to call in its aid appears less hazardous than to invoke oscillations of the earth's crust, which, in our present ignorance of the condition of the interior, may be impossible. I do not hide from myself the vast quantity of ice that is required on this supposition; nor the difficulty of accounting for its formation, and I have only been driven very gradually and unwillingly to the conclusions I now hold respecting its extent. In my last paper in this Journal I endeavoured to show that in the North Atlantic area the ice would accumulate most at the northern end of that great evaporating basin; that when it was heaped up on Greenland to such a height as to intercept all the moisture of the currents of air travelling northwards, the precipitation would take place on its southern slope only; and

* Flora Antarctica, vol. ii., p. 503.
† Trans. New Zeal. Inst., vol. viii., p. 369.

that the latter would advance southward, not only by flowing glacier-like, but by the area of precipitation itself being moved southward. A similar accumulation and extension must, I think, have taken place in the Antarctic regions. We have learnt that there is a great precipitation of snow around the edge of the ice-sheet with which the Antarctic continent, so far as we know, is covered; but an equilibrium has been attained between the forces concerned in its formation on the one hand, and its liquefaction on the other, so that it does not now advance further.

In the Glacial period that equilibrium must have been disturbed in favour of the accumulation of the ice, not necessarily by greater cold, which would tend to lessen the precipitation, but from some cause favouring a larger amount of moisture reaching the polar regions, and falling there in the form of snow. If this did occur, I believe that the ice might obtain powers of accumulation and growth, where it was fed by large evaporating areas, that might enable the ice-sheets now nearly confined within the Arctic and Antarctic circles to advance upon and invade the temperate regions; especially down the ocean depressions. I indicated some of the causes of accretion in my last paper. The currents of air travelling from the tropical and temperate zones towards the Poles, such as the counter trade winds, are charged with moisture which is precipitated when they reach colder regions or encounter mountain-chains. Confining ourselves to the southern hemisphere we find that much of the moisture falls as rain in low latitudes, yet that a certain proportion of it reaches the Arctic circle, and there falls as snow, and recuperates the ever-melting ice-sheet. Let us suppose that some change took place by which much more of this moisture would reach the southern ice-sheet, such, for instance, as would probably be effected if the obliquity of the ecliptic was increased. This, by raising the temperature of the temperate zone, would cause much more moisture to reach the southern ice-cap, and be precipitated there so as to add to its extent and height. As the edge of the ice-cap moved northward, it would gather to itself not only the moisture that would have reached it in its old position, but that which belonged to the area it now occupied.

As I remarked in my last paper on the Atlantic Glacier, but reversing the direction of its progress, it would be a ridge of ice slowly advancing northward, and appropriating the precipitation of the regions it invaded to aid in its further progress. Its advance would be due to two causes,

the flow of the ice itself, and the area of frozen precipitation being moved northward by the accretion on the northern slope of the ice-sheet; for when it had attained a certain height above the sea-level it would intercept the whole of the moisture of currents of air travelling over it, and cause its precipitation on its northern slope, so that, as it advanced further from the Pole, there would be more and more precipitation, and its progress would only be ultimately checked by it reaching a warm enough latitude to greatly increase the liquefaction of the ice. The height of the ridge would increase as it reached more temperate regions, as it would primarily depend upon the altitude to which the moisture was carried before it was precipitated, and this would be greater the lower the latitude the ridge of ice attained to.

I do not suppose that this ridge of ice, or the zone of greatest frozen precipitation, ever reached to New Zealand or to the Rio Plata, but only that the ice flowing from it did so. The zone of greatest precipitation may have been hundreds of miles farther south, and if anyone then could have stood on the edge of the great ice-sheet he would have seen to the south probably what appeared a level plain of ice reaching to the horizon, so gradual would have been its rise in that direction. M. Favre, in his elaborate account of the old glaciers of the northern rivers of the Alps, has proved that at their greatest extent the slope of the upper surface of the ice was very small, and for great distances nearly horizontal.* And Mr. Helland, describing the great ice-sheet that covers North Greenland, states that it resembles a great sea, but seems to rise slowly inland. Where the glaciers are largest on the coast the rainfall is not considerable, and Mr. Helland concludes from this that the source of the ice must be far in the interior.† In consequence of this property of flowing with a small slope the ice has been conveyed to the coast for more than a hundred miles, at least, from where it was accumulated, and the whole of the lower part of its course is far below the limits of perpetual snow. The coast of Greenland does not, therefore, owe its icy mantle to the climate there, but to the outflow from the snow-gathering grounds of the far interior. And in the Glacial period the neighbourhood of Lyons was glaciated by ice that had flowed from the far distant Alps, and which in a great part of its course was nearly horizontal on its upper surface.

* Archives des Sciences de la Bibliothèque Universelle, 1876, t. lvii.
† Quart. Journ. Geol. Soc., vol. xxxiii., p. 146.

Judging from what we know of the slope of the upper surfaces of the present ice-sheets of Greenland and Spitzbergen, and from the marks left by the old Alpine glaciers, we shall not, I think, be justified in allowing the still larger circumpolar ice-sheets a greater surface slope than 1 in 200; and supposing that the ice was 2000 feet higher than the present level of the sea, when it reached New Zealand, we should have to go back in the direction from which it flowed more than 200 miles before we reached a height of 8000 feet above the present sea-level, which may have been the zone of greatest precipitation.

Prof. Tyndall, in his well-known and often-quoted observations on the Glacial period, has so forcibly and clearly defined some of the necessary conditions to allow of a great accumulation of ice, that I may be allowed again to refer to them. He shows that the ancient glaciers required for their formation that water should be evaporated by heat, and that by lessening the force of the sun's rays we should not increase the glaciers, but cut them off at their source; and he illustrates this by referring to a distilling apparatus, and reminding us that if we wished to increase the quantity distilled we should certainly not attain our object by reducing the fire under our boiler. "It is quite manifest," he says, "that the thing most needed to produce the glaciers is an improved condenser; we cannot afford to lose an iota of solar action; we need, if anything, more vapour, but we need a condenser so powerful that this vapour, instead of falling in liquid showers to the earth, shall be so far reduced in temperature as to descend in snow."* If the theory I am advocating is correct the great condenser that exists within the Antarctic circle moved northward, and as it so moved the vapour to be condensed increased in quantity, for the reasons I have already given. It was prevented from falling in liquid showers, though it did not reach the earth as snow, but as ice flowing from the icy ridge that was fed and capped with snow.

Air, as it rises, expands, and much of its heat becomes latent, so that it is cooled according to a definite and well-ascertained law. To speak popularly, there is an inexhaustible store of cold up above, available for glaciation, if it could only be brought down.† The vapour in the air is chilled when it rises to a great height through the abstraction of its heat by the expanding air. The vapour is thus condensed, and at high altitudes frozen, so that it falls as snow.

* Heat as a Mode of Motion, p. 188.

† I am indebted to Prof. Joseph Henry, of Washington, for this suggestion.

Most of this snow is again liquefied before it reaches the earth by the higher temperature of the lower portions of the atmosphere it has to pass through. But if it be intercepted above, on a mountain range, it may accumulate there and feed glaciers that descend for thousands of feet below the snow-line. In its descent, in this form, only its upper surface is exposed to the warm air, and the lower portions of the mass are preserved as in an ice-house. Thus, glaciers may be said to bring down the cold of the upper regions of the atmosphere, and the advance of a ridge of ice from the north and south into the temperate zones, in the Glacial period, would give us the machinery for doing this on a scale commensurate with the extent of the glaciation of the two hemispheres.

According to this theory, the ice that reached the coasts of New Zealand and South America was not due to a great lowering of the mean temperature of these countries, but flowed down upon them, bringing with it the cold of regions thousands of feet above the sea-level and hundreds of miles to the southward. The theories of local glaciation require a much greater change of climate than this does, just as in the valley of Chamoinix a very considerable lowering of the present mean temperature would be necessary to cause ice to accumulate there, instead of flowing down to it from Mont Blanc. In all the treatises on the Glacial period that I have seen, the question sought to be answered is—"What causes would bring about changes of climate to allow perennial snow to accumulate on lands in temperate regions. Instead of this, I think the problem to be solved is—What are the conditions that would cause ice to be piled up around the Arctic and Antarctic circles, and gradually invade the temperate zones, bringing down the cold of the upper regions of the atmosphere as it progressed? I shall not now attempt to answer this question, but occupy what space I have left in showing how such an advance of ice explains the facts we have been considering in the southern hemisphere and some others in this part of the world.

The advance of the ice from the Antarctic regions would not be directly southward. The moist winds that fed it blew from the north-west, and its progress would be to the south-east towards its source of supply. And as the quantity of moisture in the air would be greater in some regions than in others, owing to the irregular distribution of land and water to the northward, the progress of the ice would not be equal all around the Antarctic, but some parts would

be greatly in advance of others. In both New Zealand and Patagonia there are wide plains to the east and a coast indented with deep fiords to the west. It is certainly a curious circumstance that we have similar facts of glaciation in two countries so far removed, unless they were due to general and not to local causes. The advance of an ice-sheet from the south-east accounts for the phenomena; the drainage of the eastern coasts would be obstructed, whilst local glaciers would form, or be greatly increased where they now exist, by the precipitation of frozen moisture from the south-west winds when the mountains were raised by the lowering of the ocean-level, 2000 feet higher above the sea than they now stand.

Excepting the formation of the plains, I do not know of any evidence in South America of the advance of the ice upon the eastern coast; but in New Zealand Capt. Hutton has described an immense accumulation of clay containing angular blocks of mica-schist, many of large size, on the eastern or coast side of the Taieri Plain, and extending from the Taieri River nearly to Otakaia, a distance of about 3 miles. This deposit is many miles to the eastward of any known extension of the inland glaciers, and Capt. Hutton ascribes its origin to an earlier glacier period than the last. He says that there is a similar mica-schist on the sea-coast at Brighton, but rejects the supposition that they could have come from thence, because "many of the blocks are considerably above the highest level of the mica-schists there, and there is no conceivable agency by which they could have been brought from there."* The formation of this morainic deposit, and the transport of blocks of mica-schist inland, so difficult to account for by the extension of local glaciers, is just what we might expect on the theory of the advance of the ice from the south-east. No broken shells have been found in this deposit, such as mark, in other parts of the world, the drift left by glaciers that had crossed ocean-beds; but perhaps further examination may detect them.

It may be said, in regard to the formation of great lakes of fresh water by the advance of such enormous masses of ice upon the coasts, that the cold would be sufficient to freeze the lakes themselves and change them to masses of solid ice. But a minute's consideration will remove this objection. The temperature of the ice when it reached the coasts of New Zealand and South America would be little, if anything, below the freezing-point, and it

* Geology and Gold-Fields of Otago, p. 62.

would be melting itself instead of freezing the water it came in contact with. There is no more reason for the lakes it caused being frozen than that the Manjalen Sea—which is formed in a similar way—should be so.

The advance of the circumpolar ice-sheets in the form of low ridges, rising very gradually from their outer edges and culminating at a height of perhaps not more than 8000 or 10,000 feet above the present level of the sea, and then decreasing in height towards the Poles again, in consequence of the moisture being precipitated on the outer slopes, is, I think, more in accordance with our experience than the usual form of the theory of ice-caps, which would make them highest at the Poles. The latter supposition, it has been shown, is opposed to the facts that the ice flowed northward from the northern end of Scandinavia; that the high peaks of the Lofoden Isles are not glaciated; and especially that at the present time the northern end of Greenland is much more free from ice than the southern extremity. Whilst every inlet of South Greenland is occupied by ice flowing from the interior, and breaking off into great bergs when it arrives at sufficiently deep water, in the extreme north the glaciers do not reach the level of the sea. And in the Glacial period, instead of there being more ice than now within the Arctic and Antarctic circles, there was probably much less. Within the ridge of ice that then, I think, irregularly encircled the southern hemisphere, and, in the northern, bridged across the northern ends of the Pacific and Atlantic Oceans, both plants and animals that could hybernate through the severe and long winters might have existed in greater abundance than now. Even now, in the Antarctic, behind the ridge of ice that surrounds the South Polar continent, it is possible that there may be large areas of land free from ice in summer, and supporting a flora and fauna, which, if they could be studied, might throw much light on the distribution of animal and vegetable life in the South Temperate zone. And were that barrier of ice once passed a journey to the South Pole might be found to be less impracticable than one to the northern extremity of the globe.

Whilst the physical phenomena of the Glacial period in New Zealand and South America are so similar, there are differences in its effect on the pre-glacial fauna that must be noticed. Although there was great destruction of life amongst the individuals of the great apterous birds of New Zealand, there was not the same extirpation of species as

in America, nor even so much as in Europe amongst the great mammals. Numerous bones of the moas are found in the stratified deposits of the plains, but the same species reappear in the post-glacial surface-beds. Their complete destruction did not take place at the same time as the megatherium and its associates in America, or the mammoth and the woolly rhinoceros in Europe, but must rather be correlated with that of the Irish elk and the American mastodon. The preservation of the large birds through the great vicissitudes of the Glacial period may have been owing to the following causes:—Long before the ice from the Antarctic reached the coasts of New Zealand, a great stretch of land to the north would be laid dry by the lowering of the sea-level, so that there was possibly more space suitable for their occupation during the greatest extension of the ice than now. The same lowering of the sea-level took place all over the world, and in South America a similar extension of land surface must have ensued; but there the new land, to the north of the districts glaciated or submerged beneath the waters of the great glacial lake, would be occupied by northern animals who would resist the immigration of those fleeing the catastrophes of the south. In New Zealand, also, there was not the same competition with smaller animals, which appears to have led to the extirpation of many of the bulky species of the continents during the changing conditions of the Glacial period. Had, for instance, New Zealand at that time become connected with Australia, the great extension of area thus obtained, instead of tending to the preservation of the moas, would have probably led to their extermination by bringing them into competition with the marsupials of the continent. But a channel, 2600 fathoms deep, separates the two countries; and that there was then no land connection between them is evidenced by the absence of the gum-trees and acacias of Australia from the flora of New Zealand, and of the marsupials, the cockatoos, the grass parroquets, and the pigeons of the former from its fauna.

Although the large apterous birds of New Zealand were not exterminated during the great glaciation of the country, there are other signs of the impoverishment of its fauna. About 200 species of beetles had been described in 1872, and these belonged to no less than 110 genera, giving an average of less than two species to each genus. Many of the species live also in Australia, or have nearly allied forms there. The great paucity of insects, the isolation of the species, and their affinity with those of Tasmania and

Australia, is probably due to a great destruction of the native species during the Glacial period, and the arrival since of several from the countries to the north-west. I do not know how else some of the facts can be explained, such as that of there being only eight butterflies, and amongst these some of wide distribution; and that of the Heteroptera there are thirteen known species belonging to thirteen different genera and nine distinct families.* Such gaps as these in the fauna of a country are as significant as the grooved and polished surfaces of its rocks, and the naturalist may as surely point to the evidences of the Glacial period as the geologist.

Mr. Wallace has also drawn attention to the large destruction of species of insects in the Chilian sub-region, evidenced by the great number of peculiar genera of beetles of extremely isolated forms, and I might multiply instances from the faunas and floras of southern lands, all tending to the conclusion that the southern hemisphere has been glaciated as much as, or more than, the northern; but I could not do justice to this phase of the question within the limits of this article, and I have only glanced at some of its most salient points with the object of indicating that the physical evidence of glaciation does not stand alone, but is strengthened by that of the present distribution of animal and vegetable life.

* Capt. Hutton, Trans. New Zeal. Inst., vol. v., p. 227.

IV. RECENT ADVANCES IN TELEGRAPHY.

By J. MUNRO, C.E.

OF recent years the most important development of practical telegraphy has been the duplex system of sending messages. The idea of transmitting more than one message along a single wire at once appears first to have been conceived by M. Zantedeschi, as early as 1829; but we may take it for certain that it was not until 1853, or about twenty-five years ago, that any serious attempts were made to carry it out in practice. From that time the attention of electricians, both in Europe and America, has been directed to this interesting problem, but, until the last ten years, with so little success that in the 1867 edition of Sabine's "History of the Electric Telegraph" we find the following sentence:—"Both these systems of telegraphing in opposite directions, and of telegraphing in the same direction more than one message at a time, must be looked upon as little more than 'feats of intellectual gymnastics'—very beautiful in their way, but quite useless in a practical point of view."

Experience, however, shows that it is unwise to repudiate an electric novelty; and since these words were written the duplex system of telegraphing in opposite directions has become the ordinary means of communication over thousands of miles of land-lines in England and America, and of submarine cables in Europe and the East; while the multiplex system of sending several messages in the same direction is rapidly being brought into practical service by means of the Meyer instrument in France and the telephones of Elisha Gray in America and La Cour in Denmark. Indeed, by a future combination of the duplex and multiplex systems, we may yet have a single wire transmitting as many as twenty or more distinct messages, ten either way.

In order to explain the general principle of duplex or *counter* transmission, it is necessary to be perfectly familiar with the ordinary method of simplex or simple transmission. An ordinary telegraphic circuit invariably consists of the battery or source of the electric current; the key or sending instrument, by which the circuit is opened or closed

and the current admitted into the line; next, the line or wire stretching from the Station A, where the message is sent off to the Station B, where it is received; and next, the receiving instrument at Station B, actuated by the current from the line, so as to give sensible signals. These parts of the circuit are all connected together, and at

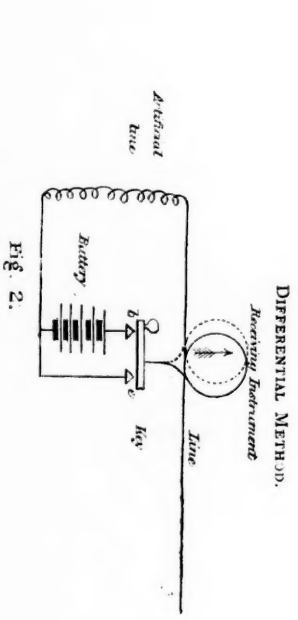


Fig. 2.

DIFFERENTIAL METHOD.

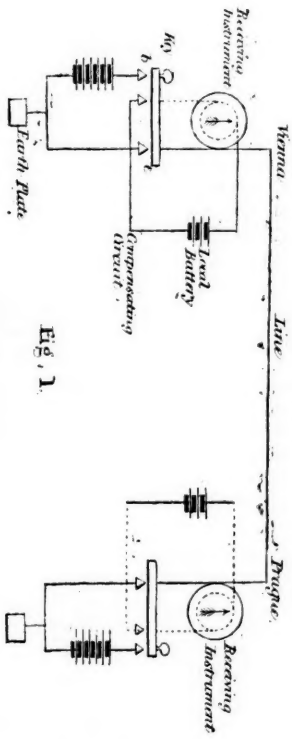


Fig. 1.

Station A one "pole" of the sending battery is put in connection with the ground by means of an "earth-plate," which is generally an unoxidisable metal, such as copper; while at Station B one "terminal" of the receiving instrument is connected, similarly, to an "earth-plate" there. A complete external circuit for the sending battery is thus formed through the line, the receiving instrument, and the

earth. This circuit is only interrupted when the lever or handle of the key is up. At such a time, of course, no current flows through the circuit. But when the sending clerk depresses the key the circuit is complete, a current traverses the line, and a signal is received at Station B. Combined according to the Morse code, such signals make up the message.

At first sight it seemed impossible that two messages could be sent, each in an opposite direction to the other, at the same time through the line. How was it possible that the messages could pass each other without interfering? If currents of equal strength were sent in opposite directions, would they not neutralise each other? In that case no current would traverse the wire at all, and what would become of the messages? Yet it is precisely on the ground that two currents of equal intensity do eliminate each other that duplex telegraphy is accomplished. The feat has been done, not by discovering or inventing a means whereby two currents could pass each other in a wire without disturbing one another, but by ingeniously joining up the sending and receiving apparatus at each end of the wire, so that the very interference of the currents, the one with the other, should cause the instruments to signal.

Such an arrangement was necessarily different from that used in ordinary transmission, as described. There was necessarily both a battery and a receiving instrument at each end; but the whole secret of the duplex system consists in the placing of the receiving instrument. All the methods hitherto invented agree in this—that the receiving instrument at either end is so placed that currents leaving the station where it is cannot cause it to signalise, whereas currents coming in from the distant station can. Again, should the currents leaving the station where it is be by any means stopped, this will have the effect of making it signalise. In describing the leading methods we shall show in each case how these conditions were obtained.

Dr. Gintl, a director of Austrian telegraphs, may be regarded as the founder of duplex telegraphy. Gintl, in 1853, described his system to the Academy of Sciences, Vienna, and practically tried it on the land-line between Vienna and Prague. Gintl's original system is shown in Fig. 1, which represents the arrangement of apparatus at Vienna and at Prague, with the line wire between. Here the receiving instrument is placed between the battery and the line, so that, when the circuit is completed, by depressing the key the current flows through the receiving instrument into the

line. With an ordinary instrument this current would produce a signal, but it is essential for duplex working that the sending from a station should not affect the receiving instrument at that station. Therefore, in order to counteract the effect of the signalling current on the instrument, Gintl employed a second smaller battery with its local or "compensating" circuit, and whenever the signalling current passed through the receiving instrument into the line he also caused a current from this second or compensating battery to pass in an opposite direction through the instrument, so as to neutralise or balance the effect of the signalling current. For this purpose the instrument was wound in opposite directions by two wires, one long and thin, represented by the full line, the other short and thick, represented by the dotted line. The long wire was in circuit with the sending battery and the line; the short wire was in circuit with the small battery and the compensating circuit. A peculiar key having double points was used for sending, so that when it was depressed it closed both the line and compensating circuits. The signalling current then rushed into the line by way of the long wire of the receiving instrument; but although it thus passed through it, it failed to make the receiving instrument signalise, because the current from the compensating battery passed simultaneously in an opposite direction through the short wire. These two currents were accurately adjusted to balance each other in their effects on the needle or indicator of the instrument. The essential condition of having the receiving instrument so placed as to be undisturbed by the sending was thus fulfilled, while at the same time it was in circuit with the line, and therefore free to receive signals from the distant station. Gintl's system had two serious defects:—In working the key in sending, the line was insulated for the moment when the lever was passing from the earth-contact, *e*, to the battery-contact, *b*. And the compensating battery was found to spend its strength quicker than the signalling battery. The harm arising from the first fault was that the received currents did not get freely to earth, and were therefore "broken up." The non-equivalence of the batteries, of course, destroyed the balance of currents on the receiving instrument, and "false" signals due to the sending were the result.

Gintl's plan, however, served as a stepping-stone to something better. Herr Carl Frischen, a telegraph engineer of Hanover, in the year 1854, greatly improved it by making the receiving instrument *differential*,—that is to say, instead of winding it with two dissimilar wires, as Gintl had done,

he wound it with two wires as similar as possible, but in opposite directions. It is unnecessary, then, to employ two different batteries. It is sufficient to split up the current from the signalling battery, sending one-half through one wire of the instrument into the line, and the other half through the other wire into the compensating circuit, which in this case must be made exactly equivalent to the line. This method of Frischen's has been called the Differential Method. It was re-invented a few months later by Messrs. Siemens and Halske, of Berlin, and is therefore sometimes called the Frischen-Siemens method.

Fig. 2 shows the arrangement of apparatus at either station. The receiving instrument is wound in opposite directions by two similar wires. The two adjacent ends of these two wires are joined to each other and to the lever of the key. The free ends are connected, one to the line, the other to the compensating circuit which—since it must be equivalent to the actual line—may now be called the

WATER ANALOGY.

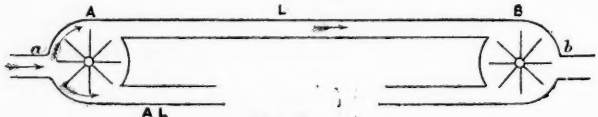


Fig 3

"artificial line." It will be seen that on depressing the lever of the key to the battery-contact the current splits at the point where the wires of the instrument branch off, and, when the artificial line is electrically equal to the actual line, exactly one-half will flow through the wire (drawn full) into the actual line, while the other half will flow through the wire (drawn dotted) into the artificial line. These two halves will counteract each other's effect on the needle, and the required electric balance will be obtained. Another advantage of this method over Gintl's was that the line-circuit is never absolutely interrupted while keying goes on; but it has this defect—that when the lever of the key rests on the earth-contact *e* the line is direct "to earth," whereas when it passes from *e* to *b* the line is to earth through the artificial line. Such a variance in the earth-contacts is attended by variance in the size of the received signals.

A convenient means of elucidating the duplex principle, in the case of the differential method, is furnished us by a

water analogy. Let us suppose that Fig. 4 is a peculiarly-shaped water-pipe. The spoked wheels or vanes in the enlarged parts of the pipe are free to rotate, and represent the needles or indicators of the receiving instruments at the Stations A and B. The long connecting-pipe, L, represents the line between these stations, and the short pipes, A L, stand for the artificial lines. Confining our attention to Station A, if we suppose that water at a certain pressure is admitted into the pipe at *a*, it will divide itself into two streams, one of which will flow over the vane into the pipe L, and the other will flow under the vane into the pipe A L, as shown by the curved arrows. If the pipes are so constructed that these two streams are equal in force the vane will remain stationary, for they will balance each other's

WHEATSTONE BRIDGE METHOD.

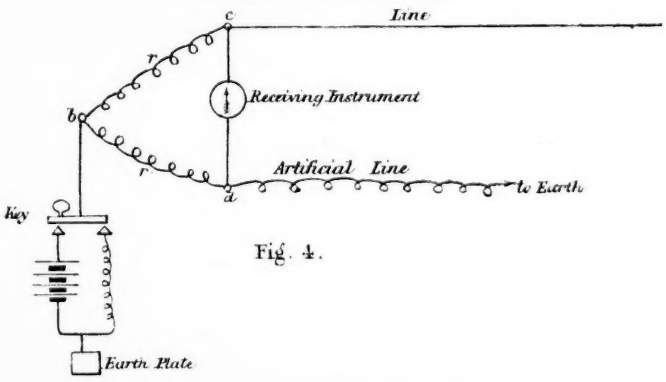


Fig. 4.

effect on it. But if by some means the stream flowing along the pipe L should be stopped, the balance on the vane would be disturbed, and it would be seen to rotate. Such an effect would be produced by admitting a like stream of water into the pipe by the orifice *b* at the other end. When the water is simultaneously admitted into the pipe at both ends the two opposing streams in the pipe L stop each other, and simultaneously both vanes move. Thus, when both stations admit water, both vanes move. We thus arrive at the two elementary cases of duplex working. The first case is when one station (say A) is sending a signal, while the other station is not. We have seen that in this case the water is only admitted at A, and the vane there is motionless; but the stream which flows along the pipe L rotates

the vane B: thus a signal is made there. This corresponds to Station A signalling Station B. The other case is when both stations are sending to each other. In this case the water is simultaneously omitted at both ends of the pipe, and both vanes rotate. This corresponds to both stations signalling each other. Out of the distinct signals the message is of course made up. In duplex sending these two elementary cases are constantly recurring, owing to the pauses between letters and words. We have, for the sake of simplicity, considered that the sending current used at each station is of the same strength and of one kind, either positive or negative. But in submarine telegraphy both positive and negative currents are used in sending, so that a third case has to be added to the above—namely, when Station A is sending a positive and Station B a negative signal. A little reflection will, however, show that this is merely a variety of the second case. It will be seen, too, that the principle still holds good when the batteries used at the two stations are of unequal strength; for the effect will only change in degree.

Immediately after the invention of the differential method there was a great deal of activity displayed in duplex experiments, both at home and abroad, especially in Germany and America. The system was even in practical operation on many of the Prussian land-lines, but it gradually fell into disuse, chiefly through the difficulty in maintaining the equivalence of the real and artificial lines, owing to imperfect insulation of the wires. Some desultory progress was still exhibited here and there, however, showing that the idea was only in abeyance until circumstances were more favourable to its development. The most noticeable of these improvements was the plan of putting the receiving instrument in the diagonal wire of a Wheatstone balance formed of the actual line, the artificial line, and two other proportional resistances. This has been called the "Wheatstone Balance Method." It originated with M. Maron, of Berlin, in 1863. The arrangement of apparatus at each station is shown in Fig. 4. The proportional resistances, $r\,r'$, are usually made equal to each other, so that by the principle of the Wheatstone balances the artificial line must be made equal to the actual line. When this is so there will be no effect on the receiving instrument placed in the diagonal $c\,d$ when the sending key is depressed and the current from the battery admitted into the system. The current will divide itself at the point b, and one-half will flow by the branch r into the real line, and so on to the distant

station, while the other half will flow by the branch r' into the artificial line, and so to earth. These currents being of equal *potential* as they pass the points c and d, no cross current between them will flow through the diagonal and the receiving instrument. A balance on the receiving instrument will be obtained, and the sending at the home station will not affect the receiving instrument there. The sending at the distant station, however, will still have control over the home instrument, for by the stoppage of the line-half of the home-sending current the balance will be disturbed, and the home-receiving instrument will signalise.

We have already seen, by help of the water analogy, in the case of the differential method, how that when only one station is sending a current only the receiving instrument at the station it is sent to receives it; and that when both stations simultaneously send currents, both instruments signalise. The water analogy can also be readily applied to elucidate the same results in this method by supposing the real line and the artificial line to represent two equal streams of water flowing down equal channels. The conditions are so arranged that there is the same volume and head of water (potential) at the points c and d, and the same rate of fall of the level of the channels. In such a case, were a cross channel to be made between c and d, and a mill-wheel placed in it, there would be no cross current in it to turn the mill. But if by some means, such as shutting a sluice-gate, the stream in the line were suddenly dammed back, then the level of c would rise—a cross current would set in towards d, turning the wheel on its way. The act of sending at the distant station is comparable with the closing of the sluice.

The next important advances in duplex telegraphy were made in 1868, by Mr. Joseph Barker Stearns, an American electrician, who has done much to place it on its present footing. Stearns's principal improvements were—the use of a peculiar key, which permitted the line always to be "to earth" through a constant resistance, and the addition of a condenser or other inductive apparatus to the artificial line, so as to imitate the sensible induction of the earth on long land-lines. Stearns applied these improvements to both the differential and Wheatstone balance methods. Fig. 5 shows their application to the former. He found that on land-lines of over 400 miles in length the sudden static charge due to the earth's induction caused a sudden "kick" or false signal on the receiving instrument; and in order to counteract it and secure the perfect balance required, he

gave the artificial line an equivalent inductive capacity, and produced a counterfeit "kick" in the opposite direction, by the addition of the condenser. Stearn's system has achieved a wide application in the United States.

STEARNS'S KEY.

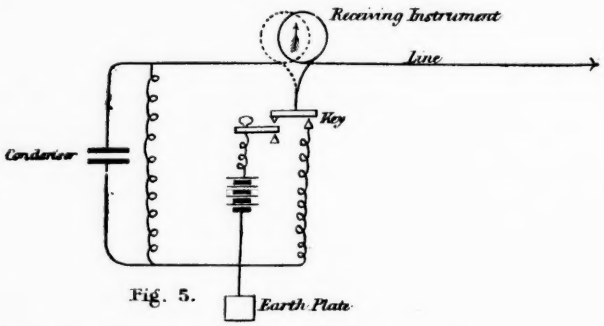


Fig. 5.

Another successful duplex method on land-lines is that of Mr. G. K. Winter, of Madras. It is called the method of "opposed batteries," and consists essentially of the "differential method;" but whereas in the latter the battery circuit is only completed in the act of sending a signal, in the former the circuit is closed, so that the batteries at each station oppose each other through the line.

Stimulated by their successes on land-lines, both Winter and Stearns, as well as others, in 1873 began to turn their attention to the far more difficult task of duplexing submarine cables. The marked inductive effect of the earth on a submarine cable—which resembles an attenuated Leyden jar—greatly complicated the requirements of the balance. The electric signal travels through a cable in the form of a wave, and is sensibly retarded by the earth's induction.

How to produce an artificial line which should be electrically equivalent to the actual cable in all its effects on the current. This was one way of solving the problem. Another lay in finding some contrivance or device (mechanical or otherwise) which should counteract the effect of the prolonged inductive charge of the cable on the receiving instrument, so as still to produce a balance.

As early as 1862 Mr. Varley had invented an artificial cable, for use as a circuit on which to test the working of

telegraphic receiving instruments. It consisted of a combination of resistance-coils and condensers—the resistance-coils, of course, imitating the conductor of the cable, and the condensers its inductive capacity. Such an artificial cable is shown in Fig. 6. The condensers, or accumulators

VARLEY'S ARTIFICIAL LINE.

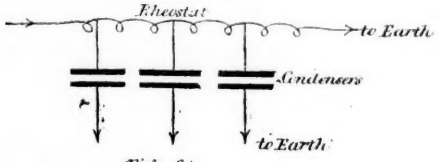


Fig 6.

as they are sometimes called, are regularly distributed along the rheostat. The current is made to pass through the rheostat or resistance-coils to earth, as through the conductor of a cable, and in its passages it charges the condensers, which in this come to imitate the induction of the earth.

Stearns made use of Varley's artificial cable in his experiments in submarine duplex. He modified it to a slight extent by attaching all the condensers together at one point of the rheostat, and inserting resistances between them, so as to regulate the charges which they took up. For instance, as shown in Fig. 7, he would insert an adjustable resistance-coil between the rheostat and a condenser, and another coil between two condensers themselves. He had thus a means

STEARNS'S ARTIFICIAL LINE.

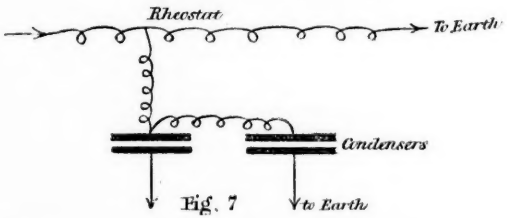


Fig. 7

of controlling the charges of the condensers so as to bring the artificial line, if possible, into harmony with the cable. But experience has shown that such a combination of condensers and resistance-coils does not approximate to a cable sufficiently for the purposes of practical duplex working on

long submarine cables. Nevertheless, by the help of the *devices* above mentioned, it is probable that cables of a considerable length might be *duplexed* with an artificial line of this kind. As yet, however, this has not been accomplished, except experimentally.

What is now known as Muirhead's system is really the first successful method of working submarine cables on the duplex principle. Mr. John Muirhead, jun., first patented this system in 1874, and since then it has been rapidly developed and applied, chiefly by Dr. Muirhead and Mr. Herbert Taylor, C.E. The system includes both a novel artificial line, or model cable, and various adjustments or devices for bringing the artificial line into close equivalence with the cable, and for perfecting the balance. Muirhead's artificial line aims at complete equality with the cable, as far as it can practically be done. Like a real cable, it is an intimate union of resistance and capacity, and is called an "inductive resistance." It is formed "by taking two strips of tin-foil, and laying one over the other separated by an insulator, such as paraffined paper. One strip forms the conducting circuit of the artificial line; the other forms the outer or inductive coating, and is connected to earth. The current is passed through the conducting strip, and exposed

MUIRHEAD'S ARTIFICIAL LINE.

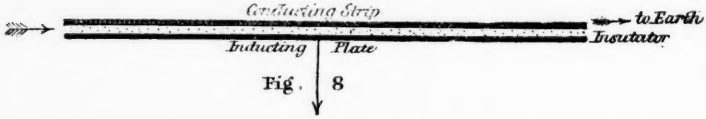


Fig. 8

throughout its entire length to the induction of the other strip or sheet in the same way that the current in the conductor of a cable is subjected to the induction of the earth "throughout its entire length." This is shown in Fig. 8, where the arrows represent the local current passing along the conducting strip, while being at the same time retarded by the induction of the other strip, or inducting-plate, which is connected to earth. This model cable can be made to have the same resistance, capacity, and even leakage, per knot that the actual cable has, so that it may be practically equivalent to the actual cable. For very long lines, however, and with such delicate receiving instruments as Sir William Thomson's "mirror galvanometer" and "siphon recorder," it is in general necessary to refine upon the balance by means of extraneous adjustments. It may be

said, however, that the more nearly the model cable is made to equal the real one, the less needful is any other help. The use of such additional devices is, as we have said, to counteract the "kick," or false signal, on the receiving instrument, due to a want of perfect accord in the balance between the real and artificial lines. Thus it will be seen by a reference to Fig. 4 that if there is any difference between the rush of electricity into the cable at *c* and the rush into the artificial line at *d*, a momentary pulse will traverse the receiving instrument and jerk the needle; this will take place at every signal sent from the station. The needle thus "kicked" by the home sending is of course unable to deliver the message from the distant station. It is comparatively easy to get rid of the violence of the kick and to reduce it to a slight jar or tremor, but it is a difficult matter to eliminate it altogether. This, however, has been practically achieved by Muirhead's system.

The principal device for obviating the "kick" is to counteract its effect on the instrument by superposing an equal "kick" in the opposite direction. This can be done by allowing a battery-current to flow for an instant through an extra coil of the receiving instrument. But it is generally effected either—

By connecting a condenser to the instrument, so that a sudden inductive charge into it shall produce the kick required; or—

By using a "secondary battery" (made of a series of plates of lead or of platinum in a weak solution of sulphuric acid) in the same way as the condenser; or—

By employing an induction-coil and placing the primary in circuit, somewhere, with the signalling-current, while the secondary is connected to the receiving instrument; so that the sudden induced currents generated in it by each signal-current may produce a "counterfeit kick" in the opposite direction to the real one, and so completely neutralise it.

But although these plans of neutralising the kick by an equal and opposite one are doubtless serviceable, they introduce complications into the balance which are to be avoided, if possible, in practice. By the use of the inductive-resistances, and a simple but effectual adjustment which is applied to them, the kick is practically reduced to *nil*, and a perfect balance obtained, so that constant sending in opposite directions can go on simultaneously with signals as beautiful as in the ordinary method of working.

XUM

Muirhead's system was first experimentally tried, by Dr. Muirhead and the writer, on the Marseilles to Bona (Algeria) cable of the Eastern Telegraph Company, in 1875. The following year it was established for the regular traffic on the Marseilles to Malta section, and on the Suez to Aden or Red Sea cable, which is 1460 miles long, and in an electrical sense one of the longest of existing cables. Early in this year it was established on the Aden to Bombay section, a cable of 1817 miles long. There are thus upwards of 4000 miles of this Company's lines to India worked on the duplex system, and it has been given out that the Company hopes this year to apply it to all their cables.

The duplex system of transmission is without doubt a great practical gain, since it doubles the carrying capacity of a line, and makes it speak, as it were, with two tongues. With such facts as the above before us we can no longer regard it as a "species of intellectual gymnastics." It has been brought at last to a serviceable issue, and bids fair to become a considerable public benefit.

Quadruplex Telegraphy.

Very soon after Dr. Gintl, by his invention of the duplex system, had shown the feasibility of simultaneously transmitting two messages counter to each other along one wire, electricians engaged their minds with the allied problem of transmitting two messages simultaneously in the same direction. And they were not backward in foreseeing that by the union of both methods a system of quadruplex telegraphy would be placed in their hands. On the 27th of October, 1855, Dr. J. Bosscha, jun., of Leyden, at a meeting of the Royal Academy of Sciences, Holland, described a method he had invented for the simultaneous sending of two messages in one direction along one wire, and likewise pointed out that, by the union with it of the Frischen-Siemens duplex system, a means of sending four messages simultaneously was provided. On the 31st of the same month Dr. J. B. Stark, of Vienna, published his method of double-sending in the same direction, and concluded his account as follows:—"With the method of double transmission in the same direction we may also combine that of counter-transmission (*gegensprechen*), and hence arises the possibility of simultaneously exchanging four messages upon one wire between two stations, which will, however, hardly find any application in practice."

Following Stark and Bosscha, other electricians in Europe during the following year invented methods for effecting the same purpose—notably Drs. Werner Siemens, Kramer, and Bernstein, of Berlin. All these early methods agree in principle, although they differ in detail. The problem of double transmission implies the use of two keys or sending instruments at one end of the line, and of two or more receiving instruments at the other. The use of two keys simultaneously involves four combinations of their action, namely:—

1. When No. 1 is closed and No. 2 is open.
2. When No. 2 is closed and No. 1 is open.
3. When both are closed.
4. When both are open.

These four conditions of the key should give rise to four distinct electrical conditions of the line, which are capable of being interpreted by the receiving relays. In the method of Bosscha, Kramer, and others, these electrical conditions were, for—

1. A positive current of strength 1 sent.
2. A negative current of strength 1 sent.
3. A positive or negative current of strength 2 sent.
4. No current sent.

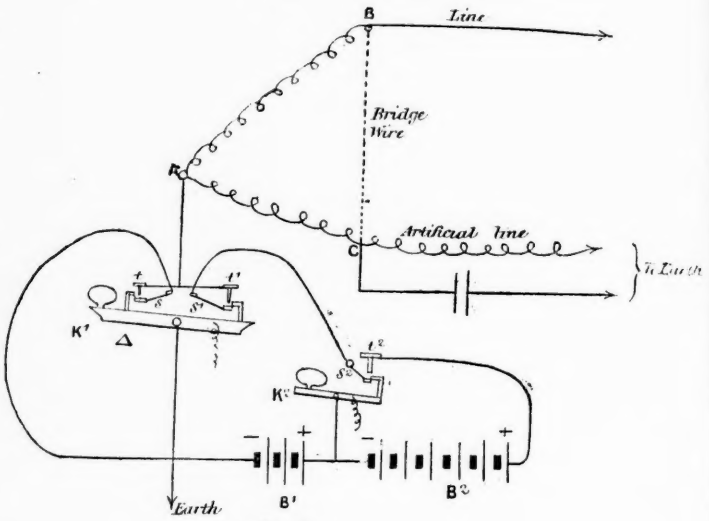
They were interpreted at the receiving end of the line by one polarised and two neutral relays. In the method of Stark, Siemens, and others, the electrical conditions were—

1. A positive current of strength 1 sent.
2. A positive current of strength 2 sent.
3. A positive current of strength 3 sent.
4. No current sent.

These were interpreted at the receiving end of the line by three neutral or unpolarised relays of different degrees of sensitiveness.

The defects of these early systems lay in part with the keys and in part with the receiving relays. When the keys were being worked there were momentary interruptions of the line-circuit in changing from one contact to another. It was found to be a difficult matter to keep the three relays at their proper degree of sensitiveness in Stark's system. In Bosscha's system the sudden reversal of magnetic polarity in the neutral relay, caused by the reversal of currents, had the effect of breaking up the signals of the neutral relay. These difficulties, however, might all have been overcome had it not been for the additional disturbances or "kicks" due to the static induction of the line. These

QUADRUPLEX TELEGRAPHY.



Fig, 9.

QUADRUPLEX TELEGRAPHY.

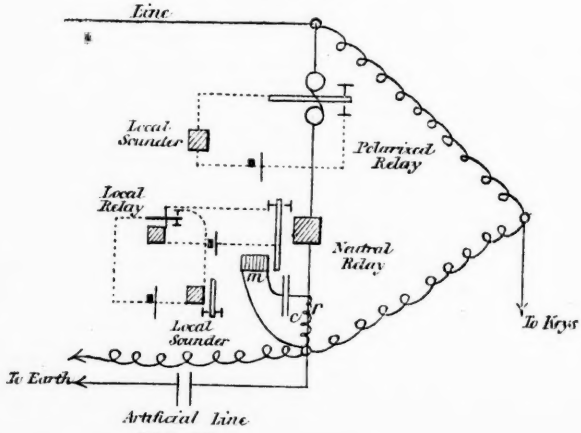


Fig. 10

proved fatal to the early duplex systems, and although there was up till 1865 considerable ingenuity shown, nothing practical was done in double transmission.

The recent revival of duplex, however, again revived the hope of double transmission and of quadruplex. In 1872 Mr. Stearns made duplex a practical success by applying the condenser to the artificial line, and thereby overcoming the "kicks;" and in 1874 Mr. T. Alva Edison, of Newark, New Jersey, and Mr. G. B. Prescott, while testing Stearns's system, invented the first practical system of quadruplex. It was worked, in the same year, on the New York to Boston line, a distance of 240 miles. Since then it has been introduced on several long lines in the United States, including the line from New York to Chicago, which is nearly 1000 miles in length. On this line there is a "repeater" at Buffalo. The speed of working is about 120 words a minute.

The basis of Messrs. Prescott and Edison's quadruplex system is the "Wheatstone bridge" duplex system. With this is united their system of double transmission, which resembles the early method of Bosscha, but differs from it in many important points. To understand their system it is only necessary to consider their plan of double transmission, since we know that the mere duplexing of this arrangement ensures quadruplex. The combination of currents produced by the two keys are for—

1. A positive current of strength 1 sent.
2. A negative current of strength 3 or 4 sent.
3. A positive current of strength 3 or 4 sent.
4. A negative current of strength 1 sent.

There is a practical advantage in this system over the earlier ones, in the greater disparity between the different currents. To interpret these currents a Siemens polarised relay and a neutral relay are used. The neutral relay will work by currents, either positive or negative, provided they are sufficiently strong. The currents sent by 2 and 3 work this relay. The polarised relay depends for its working only on the *kind* of the current, not on its strength. Thus a negative current keeps the armature or tongue of the relay against its back stop, thereby keeping the local circuit open, whereas a positive current causes the tongue to close the local circuit. This relay therefore works with 1 and 3 currents.

Fig. 9 represents the arrangement of the keys and batteries for sending. A B C is the "Wheatstone bridge," with

the bridge wire B C, in which the receiving relays are placed. K_1 and K_2 are the signalling-keys. B_1 and B_2 are the batteries. K_2 is an ordinary "single-current" key, whose function is simply to add or take out the battery power, B_2. K_1 is a "double-current" key. When in its position of rest, as shown, a negative current of strength 1 continually flows to line from the − pole of B_1, through the spring *s*, and *t*. This negative current is too weak to close the neutral receiving relay, and it is of the wrong kind to close the polarised relay. When, however,—

Case 1, this key is closed, the positive current from B_1 flows to line through the spring s_1 and t_1. It is too weak to close the neutral relay, but it closes the polarised relay.

In Case 2, when K_2 is closed and K_1 is open, the + pole of B_2 is to earth through t_2 and springs s_2 and s_1, while the − pole of B_1 is to line through *s* and *t*. Thus both batteries act conjointly, and a negative current of triple strength flows through the line. This has the effect of closing the neutral relay and leaving the polarised relay open.

In Case 3, similarly, it will be found that a positive current of triple strength enters the line. This closes both relays.

In Case 4, when both keys are open, as shown, the − pole of B_1 is to line, and a negative current of strength 1 flows. It has the effect of keeping both relays open.

Fig. 10 represents the arrangement of receiving relays with their local circuits. R_1 is the polarised and R_2 the neutral relay. The local circuit of R_1 is made up of a local "sounder" and actuating battery; that of R_2 is made up of a local relay battery and "sounder." The addition of this local relay is to overcome the defect experienced in the early systems of the breaking-up of signals in the neutral relay due to sudden reversals of current in the line. A little consideration will show that the current may be reversed in the line by the key K_1 while a signal is being made on K_2. This reversal of polarity in the relay involves an intermediate moment of no polarity, during which the tongue t_2 will fall away from its contact. This causes a break in the signal made by the local sounder. The intervention of a local relay, however, was found to delay the action of the sounder, so that this interval of falling off could be bridged over, so to speak. On long lines the static induction, by delaying the change of polarity in the relay, extends this interval so

much that the local relay no longer avails, and recourse must be had to another device. This consists in adding a derived or loop circuit to the bridge wire, consisting of a condenser, *c*, electro-magnet, *m*, and adjustive resistance, *r*. The electro-magnet is set so as to act upon the other end of the tongue of the neutral relay, as shown. In this way, when a change of current takes place, the discharge from the condenser through *m* serves to keep the tongue in its place until the critical time is past; *r* is merely for adjustment. By these ingenious contrivances Messrs. Prescott and Edison have rendered their quadruplex system a practical success. Within the last few years other systems, more or less similar, have been devised, but as yet theirs is the only one of note.

Telephonic Telegraphy.

a. *The Tone Telephone.*

The last five years have seen the practical development of this interesting branch of telegraphy. For many years back attempts have been made to transmit musical tones or articulate sounds to a distance by means of electricity, but the results were at best only hopeful until, in 1860, Herr Phillip Reis, of Homburg, profiting by the researches of Wertheim, Marian, and Henry, invented the first telephone. In Reis's telephone a stretched membrane is set into vibration in unison with the sound to be transmitted, and, by a little contact-piece which it carries, this vibrating membrane is caused to interrupt the electric current circulating in the line. The current so interrupted is utilised at the distant station to produce a sound similar to the original sound at the near or sending station. To produce such a sound Reis made use of Page's discovery, that an audible "click" accompanies the demagnetisation of a bar of iron inserted in an electro-magnetic helix. He surrounded an iron wire with a helix, and caused the interrupted current from the sending station to pass through the helix to earth. At every interruption a distinct sound was given out by the iron core, and the joint effect of these reproduced a note of the same pitch as that sounded at the sending station.

In telegraphy, however, the telephone only acquires special importance when it is regarded as a means of multiplex transmission. A single message by telephone can have small advantage over the ordinary methods; so Reis's telephone was neglected as a kind of scientific curiosity.

Ten years later, in 1870, Mr. Cromwell Fleetwood Varley, F.R.S., appears to have first clearly designed the employment of the telephone in the transmission of several messages simultaneously on one line wire, by means of separate notes. Varley's patent of that year is full of most ingenious plans and contrivances, not only for the sending and receiving of telephone messages, but for rendering the vibratory electric signals visible, both temporarily, by light, and permanently, by recorded marks on moving paper. The principles of later inventions are to be found here, and even some of the details. But Varley seems to have left the carrying out of his system in abeyance; and the merit of practical success belongs to the systems of M. Paul la Cour, of Copenhagen, and Mr. Elisha Gray, of Chicago.

The principle of all these systems is that a vibrating body, such as a tuning-fork or membrane, emitting a certain note, shall be caused at each vibration to interrupt an electric

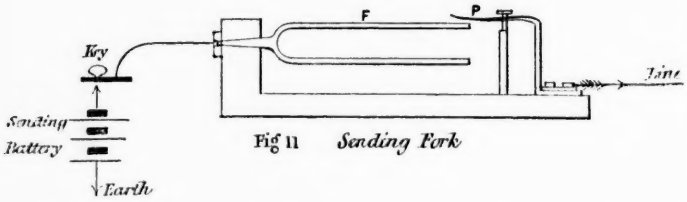


Fig 11 *Sending Fork*

current in the telegraph line; and the current so interrupted shall, at the distant station, be made to set a corresponding body in vibration, so as to reproduce the original note there. Of course it is possible, by having a number of vibrators at the sending station and a corresponding number of vibrators in unison with them at the receiving station, to give rise simultaneously to several distinct sets of electric vibrations in the line wire, and to reproduce several distinct notes at the receiving station. In this way is multiplex transmission rendered feasible.

M. Paul la Cour's English patents bear date 1874 and 1876. Briefly, his system is as follows:—Fig. 11 represents his sending arrangement. F is a tuning-fork which is put into vibration, and makes contact once every vibration with the contact-point P. The fork is connected, through the signalling-key, with the sending-battery, and the contact-point P is connected to the line. Here the fork is simply shown, and must be started by hand; but in his later patent

M. la Cour provides, by means of electro-magnets, that the fork shall be maintained constantly in a state of vibration. In either case the current sent into the line by depressing the signalling-key is interrupted by the fork an equal number of times per second that the fork vibrates. This intermittent current traverses the line to the distant station, where it is passed "to earth" through the receiving apparatus shown in Fig. 12. This consists of a fork similar to the first, and vibrating the same note. Each leg is surrounded by a helix of wire. Two other helices are placed upright, one on each side of the ends of the legs; they are fitted with iron cores, and adjustible poles, *n*, *s*, and are in fact electro-magnets. These four helices are joined up in series, so that the line-current passes through each in turn. It

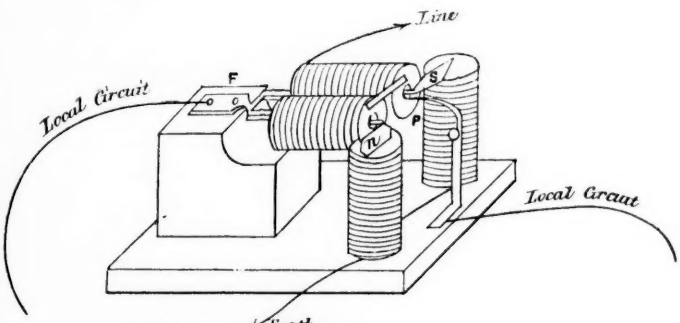


Fig 12. *Receiving Fork*

magnetises oppositely the legs of the fork, which are attracted by the contiguous poles of the electro-magnets. The legs of the fork are thus pulled apart, and let go with each pulsation of the intermittent current. In this way the fork is put into audible vibration.

These vibrations are also turned to the actual recording of the message in permanent marks, by means of a local circuit and Morse or other recorder. A fine metal point, P, is brought very near to one leg of the fork, so that when the fork vibrates this leg comes into contact with the point, and completes the local circuit through the fork, local battery, and recording instrument.

M. la Cour's first experiments were made in June, 1874, on a short line in the neighbourhood of Copenhagen, and in November of the same year he succeeded in working

successfully from Fredericia, in Jutland, to Copenhagen, a distance of 390 kilometres.

Mr. Elisha Gray's first English patent bears date of 1874, a few months before M. la Cour's; but in it there is no mention of multiplex telegraphy. Subsequent patents, in 1875 and 1876, describe the development of his system and its application to multiplex signalling. His principal methods of sending are represented in Figs. 13 and 14. In Fig. 13 the vibrator is a tuning-fork or reed, R, placed between two electro-magnets, A and B, which can be adjusted by the screws D and G to or from the reed, so as to have greater or less power over it. The reed is barbed with

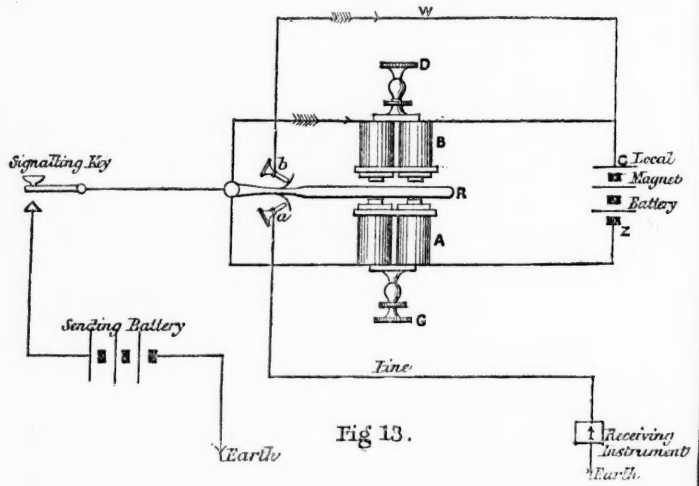


Fig 13.

two short springs, which play against the adjustible contact-points *a* and *b*, making contact with them alternately as the reed vibrates. The reed is perpetually maintained in vibration by means of a local battery and the electro-magnets in the following way:—The local battery is connected up, as shown, through the electro-magnets and the fixed end of the reed. When the reed vibrates so as to make contact, by means of its spring, with *b*, and break contact with *a*, the electro-magnet A is in circuit and actuated by the current, while the magnet B is cut out of circuit by the short-circuit wire W. The magnet A therefore exercises a pull upon the reed R, which assists the reed in vibrating towards

it, and breaking contact with *b* while making contact with *a*. This, while keeping the electro-magnet A still in circuit, also puts the magnet B in circuit. In this way the electro-magnet B is alternately thrown in and out of circuit, and

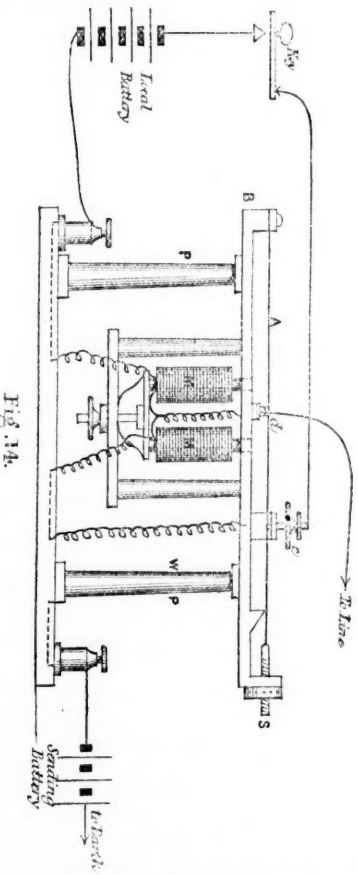


Fig. 14.

the effect of this on the reed is to keep up its vibration. The reed, thus kept vibrating by the electro-magnets and local battery, is used to interrupt the line circuit and sending current. The sending battery is connected up, as shown, through the signalling-key to the reed, and by the reed-

spring and contact-point *a* to the line. At each vibration of the reed the contact between the spring and *a* is made and broken, and the circuit opened and closed. Whenever the signalling-key is depressed, therefore, an intermittent current enters the line.

Mr. Gray also procures the intermittent current by means of a vibrating bar or string, as in Fig. 14, where A is a thin steel bar stretched by the screw S, and carried by a fixed metal bar or frame, B. The bar A vibrates between upper and lower contact-points, *c*, *d*. M M are two electro-magnets whose poles are let through the bar B, so as to act upon the vibrating bar. P P are pillars supporting the whole. In this arrangement a local battery is also employed to set and maintain A in vibration: this is done, however, in the act of signalling. On closing the signalling-key the local current passes to the upper contact *c*, against which the bar A rests. From thence it passes, by means of the bar B and wire W, through the magnets M M, completing its circuit. These magnets then attract the bar downwards in its middle, plucking it away from contact with *c*, and bringing it into contact with *d* underneath. The local circuit being thus broken, however, the bar springs back again into contact with *c*, to be plucked down again as before. In this way the bar is started vibrating. The line circuit is made through the lower contact *d*, the bars B and A, and the pillar P. At every vibration of the steel bar A it is interrupted, and an intermittent current set up in the line.

In both of these methods the number of intermissions in the line current will correspond with the number of vibrations of the vibrators. By employing in the first method reeds of different pitch, and in the last bars or strings of different dimensions and tension, distinct intermittent currents will be produced.

The principal receivers for interpreting these currents into distinct audible sounds are represented in Figs. 15 and 16. In Fig. 15 M is a double electro-magnet supported over a resonance pipe closed at one end. The soft-iron armature of the electro-magnet, *t* T, is rigidly fixed to one pole at *t*, the other end being free to vibrate in front of the other pole at T. When the intermittent current from the line passes through the electro-magnet the free end of the armature or tongue is set into corresponding vibration, and the air-column in the resonance box, vibrating in unison with it, gives out an audible note. It is easy to see how, by employing a number of separate transmitters, such as described, to superimpose separate intermissions in the line current,

and passing the complex current so produced through as many separate receivers of this kind, that each receiver will only respond to its own particular set of intermissions. For the sounding-pipe of each receiver can be so constructed

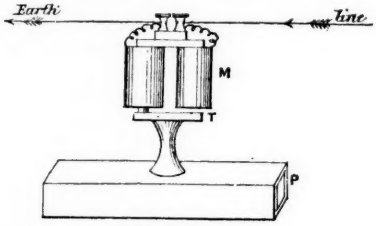


Fig 15.

as to resound only to the vibrations set up in the tongue by a particular series of intermissions. In this way several distinct notes may be simultaneously telegraphed, each note being used for a separate message.

The receiver shown in Fig. 16 has been called the "physiological receiver," since it depends for its action on the

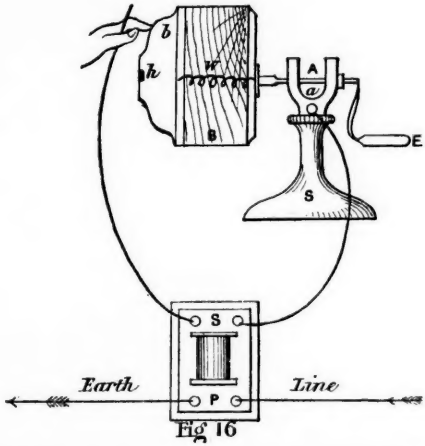


Fig 16

contact of living animal tissue with a conducting surface. It is the most interesting of the two, because its action has not hitherto been explained. If the intermittent current from the line is passed through the tissue to the conductor,

the corresponding note is faintly audible. To intensify the effect the line current, in practice, is simply passed to earth through the "primary," P, of an induction-coil, and the more intense secondary current is used. In the figure, B is a hollow wooden resonance-box, with a bulging zinc face, *b*. This box is carried by a metal axle, A, supported by a stand, S: it is rotated by turning the handle, E. The zinc face is connected by a wire, *w*, to the axle; *h* is merely an air-hole in the face of the box. One end of the "secondary coil," S, is connected by wire to the metal axle at the terminal *a*; the other end is connected to a bare wire held in the operator's hand, as shown. The operator lightly presses a finger of this hand on the zinc face, while with the other he rotates the box, and the dry rasp of the skin on the zinc surface is changed into a musical note whenever the current passes.

Like M. la Cour, Gray also provides that the vibratory current shall close a local circuit and record the message in permanent marks, by means of a Morse or other recording instrument. For this purpose he employs a receiver similar to the string-transmitter shown in Fig. 14. The line current, passing through the magnets of this instrument, sets the tense bar in vibration against the upper contact *c*, thus closing the local circuit and actuating the local recorder.

Mr. Gray's apparatus is now successfully operated over more than 2400 miles of the Western Union Telegraph Company's lines, including distances of several hundred miles. As many as four, and even eight, messages are simultaneously sent. Both he and M. la Cour are still engaged in perfecting their apparatus, and we may reasonably expect that the telephone will ere long do good service as a practical telegraph.

b. *The Articulating Telephone.*

This ingenious little instrument is the most wonderful of all the forms of telephone, and the latest, as it is the greatest, to use the words of Sir William Thomson, "of all the marvels of the electric telegraph." Its peculiar faculty lies in the transmission of promiscuous sounds. Not only does it convey the blended notes of musical instruments, but it actually reproduces the human speech. It is easy to see how an instrument like this will become practically useful. For domestic or commercial purposes, for reports of lectures and speeches it is especially fit. It has the advantage of quickness over ordinary methods of telegraphing.

In these, each letter of a word is on an average composed of three distinct signals; but in the articulating telephone a whole word is transmitted by the single act of uttering it.

Mr. A. Graham Bell, the inventor of this now-famous instrument, is, we believe, a native of Edinburgh, and is now a Professor of Boston University, and a naturalised

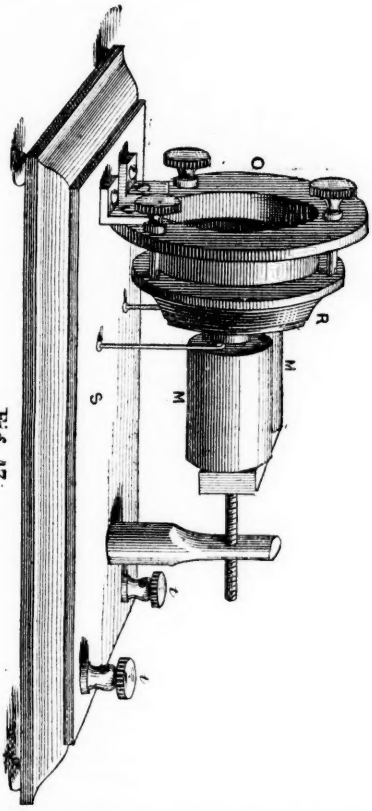


Fig. 17.

citizen of the United States. In December last he patented it in England.

The articulating telephone consists of two distinct apparatus, a sender and a receiver. Fig. 17 represents the sender, and Fig. 18 the receiver: they are both exceedingly simple. In the sender, M M are two coils of insulated wire

surrounding the two poles of a powerful permanent magnet. These coils are connected up together, and to the terminals *t t*, on the mahogany stand S. Immediately in front of the poles of a magnet is a membrane stretched on a ring, R. This membrane carries an oblong piece of soft iron cemented to it just opposite the poles of the magnet. A suitable acoustic cavity, or mouthpiece to speak into, O, fitted with three screws for tightening up the membrane, complete the apparatus. The sender speaks into the mouthpiece in an elevated voice, and the membrane, vibrating in unison, carries the piece of soft iron—which is really a movable armature—to and from the poles of the magnet. This has the effect of inducing a magneto-electric current in the coils of wire, M M, which are connected up to the line. The strength of this induced current varies continuously, "as nearly as may be, in simple proportion to the velocity of a particle of air engaged in constituting the sound." It travels along the line, and passes through the receiver at the distant station, evoking there the sounds which gave

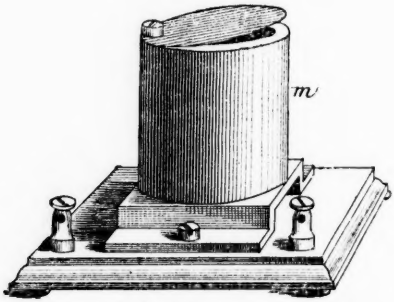


Fig. 18.

rise to it. The receiver is even more simple than the sender, and its action is the reverse. It consists of a tubular electro-magnet, *m*, encased in iron to concentrate its power as much as possible on the circular disk armature, *a*, which is so fixed as to be free to vibrate over its cavity. It is connected up in circuit with the line by the terminals shown. The induced current coming from the sending station passes through the coil of this electro-magnet, and sets the thin disk armature into sonorous vibrations, which are distinctly and clearly heard as a reproduction of the original sounds.

In his address to the physical section of the British Association at Glasgow last year, Sir William Thomson thus

related his personal experience of this speaking telegraph at the Centennial Exhibition, Philadelphia. "In the Canadian department I heard 'To be, or not to be . . . there's the rub,' through an electric wire; but scorning monosyllables, the electric articulation rose to higher flights, and gave me passages taken at random from the New York newspapers: 'S.s. *Cox* has arrived' (I failed to make out the s.s. *Cox*); 'The City of New York,' 'Senator Morton,' 'The Senate has resolved to print a thousand extra copies,' 'The Americans in London have resolved to celebrate the coming Fourth of July.' All this my own ears heard spoken to me with unmistakeable distinctness by the then circular disc armature of just such another little electromagnet as this I hold in my hand." Since then, Professor Bell has been perfecting his apparatus, and we find, on the authority of the "Scientific American," that he has recently achieved even more remarkable success. At a lecture which he delivered at Salem, Mass., a reporter of the "Boston Daily Globe" sent a verbal report of the lecture to the office of his paper in Boston, eighteen miles distant, by means of the telephone. Those reciving the message at Boston even heard, from time to time, the applause of the audience attending the lecture. From the platform Professor Bell spoke to his associate Mr. Watson in Boston. The latter then sent a telegraph message in musical notes, and also a tune from an organ, which were distinctly audible to the audience. On being asked for a song, Mr. Watson complied with "Auld Lang Syne," and finally made a speech, the words of which were heard by all present in the hall, as the applause testified. Mr. Watson then returned thanks, and the meeting ended, we are told, by those at Salem joining in the national anthem, "Hail, Columbia," with those at Boston. In view of these striking facts, it is hardly going too far to anticipate the time when, from St. James's Hall as a centre, Mr. Gladstone will be able to speak to the ears of the whole nation, collected at a hundred different towns, on Bulgarian atrocities, or some other topic of burning interest. Nor need we despair of seeing Herr Wagner from his throne at Bayreuth, dispensing the "music of the future" in one monstre concert to St. Petersburg, Vienna, London, New York—in short, to all the musical world at once.

V. ON THE PRESENT CONDITION OF CHILE.

THOUGH Chile has only had a settled Government for some twenty-five years, the country has now been autonomous for about half a century, and it will, therefore, not be uninteresting at the present time to consider briefly the results which have been achieved in that limited period. With this end in view we propose in this paper to abridge some notes from an exhaustive report, recently furnished to the Foreign Office by Her Majesty's Secretary of Legation at Santiago de Chile, on the progress and general condition of the Republic—a report which reflects great credit on its writer, and, apart from a few defects in the arrangement of its details, is one of the most complete that we remember to have met with in a long experience of such like documents.

We need not dwell at great length on Mr. Rumbold's introductory remarks on the geographical position and physical configuration of the country, for they are more or less familiar to all; but we may mention that Chile claims to extend from the 24th degree of southern latitude to Cape Horn, with a coast-line ranging over 2000 miles, the greater part of her territory—from the province of Aconcagua southwards—being describable as one broad valley running due north and south, with narrower lateral and intersecting valleys, each of which rises step-like above the other to the foot of the giant wall of the Andes. However genial the climate and fertile the soil, the extent of land available for cultivation is necessarily limited by the large proportion of hills and rocks, and by the extensive desert tracts of the northern districts. At the same time the natural declivity from the mountain to the ocean, distinctive of the whole country, as well as its inconsiderable breadth (nowhere much exceeding 120 miles), greatly facilitate communication with the coast at all points. It is thus marked out by Nature for easy exportation of its own produce, and is equally convenient of access to the sea-borne produce of other nations. The Chileans would therefore seem destined to become both an important agricultural and an important commercial and maritime community—points of resemblance with ourselves which they no doubt include in their claim to be considered "the English of South America." Mr. Rumbold does not

profess to make any attempt at a detailed description of Chile, but briefly classes it in the following three main divisions :—

1. The large northern provinces of Atacama and Coquimbo, the former of which is almost entirely made up of sandy deserts replete with mineral wealth. Its richest mines are to be found between Caldera and Mejillones de Bolivia, over a district covering 240 miles. Here are the silver mines of Puquios, Tres Puntas, Chañarcillo, Chimbero, and numerous others, and here, too, are the newly-discovered mines of La Florida. In this vast region every conceivable mineral product is found; silver in abundance, and copper; gold, iron, lead, nickel, and cobalt; not to mention great *pampas* or stretches covered with nitrate of soda, rock-salt, and borax, these latter being of recent discovery, and when analysed found to contain a large proportion of iodine. The great discoveries made at Caracoles—which, it may be well to note, is not in Chilean but in Bolivian territory—have somewhat diverted public attention from the mines of Atacama, but their wealth is none the less remarkable, and it will suffice to say of them that in the thirty years period 1843 to 1873 they yielded over 200,000,000 dollars' worth of mineral produce, or an average annual yield of about £1,320,000. The southernmost portion of the province (the department of Freirina) bears a different aspect from the rest, for it contains the Vale of Huasco, which is renowned as one of the best-cultivated districts of Chile, but upon which the desert is now said to be encroaching at an alarming rate. The department is, above all, the greatest copper district in the world, and its rich mines of Carrizal are well known. The province of Coquimbo, too, abounds in this metal.

2. The limits of the second, or central, division of Chile would be best marked by the Rio Illapel, or Choapa, and the Rio Itata. It comprises the provinces of Aconcagua, considered the garden of Chile; Santiago, with the large and handsome capital city of the same name; Valparaiso, with the largest commercial emporium of the South Pacific; Colcagua, Talca, Maule, and Nuble. It is the heart and soul of the whole country, containing its largest estates and most important towns, yielding its most abundant produce, and supporting the bulk of its population. It is distinctly agricultural, as opposed to the mining north and to the wilder and more pastoral south.

3. The third, or austral, division of Chile, *el Sur*, commences—according to Mr. Rumbold—at the Rio Itata, and

includes the province of Concepcion, the so-called province of Arauco, and the provinces of Valdivia, Llanquihué, and Chiloé. This extensive region is but thinly peopled, and only in part subdued, the greater portion of Arauco, and much of Valdivia and Llanquihué, being still in the hands of the independent aboriginal Araucanian tribes that held them before the Conquest. It is a region eminently adapted for stock-breeding and other pastoral pursuits, but it also includes large coal-fields. Its moist and temperate climate, excellent soil, numerous and commodious harbours, beautiful rivers, with banks clothed down to the water's edge by primæval forests full of valuable timber, point it out as especially favourable to emigration from Europe.

Before quitting this part of our subject, a few words must be devoted to the colony of Punta Arenas, or Sandy Point, in the Straits of Magellan, which is the most southern civilised community of the globe. The station is described on authority as peculiarly healthy; the mean temperature of the whole year is 44·8° F.; that of spring being 45·9°; of summer, 52·6°; of autumn, 44·6°; and of winter, 35·8°. The settlement now produces enough to support itself; its shores are covered with forests of Antarctic beech, and it is expected that a large and profitable trade in railway-sleepers and other timber may be developed with the countries on the River Plate: it likewise contains coal-mines, which promise well, and which may not impossibly open out an important future to the place as a coaling-station; mines of copper are said to exist, and gold is found in the Rio de la Mina to the north of the settlement; but the sources of wealth more special to the colony are its cod-fisheries and trade in guanaco skins, and in ostrich skins and feathers. In 1868 the population was 195, and in 1875 was 1144.

Census.—According to the preliminary Report of the Commissioners the population actually found in Chile on April 19, 1875, numbered 2,068,424 souls, being an increase of 249,201, or 13·7 per cent, in ten years. The difficulty of counting a people thinly spread over an immense territory, and in many cases disinclined to help the persons charged with the operation, was such that the Census Commissioners think it safe to add at least 10 per cent for omissions, and they accordingly put the total figure of the population at 2,319,266. Some curiosities of the census are worth noting. As instances of the vague distrust with which the process was regarded in many rural districts, it is stated that at Conchali, in the department of Quillota, nearly all the *peones*, or labourers, on one estate left their huts on the eve

of the 19th of April, and took to the hills for refuge. At Romeral a similar exodus occurred, and only 500 inhabitants were found instead of at least 1200, known to belong to the place. The prevailing idea appeared to be that a forced *levée en masse* was intended, or that the population was being counted with a view to the imposition of a poll-tax. As an illustration, on the other hand, of the little intelligence sometimes shown in collecting the necessary data, it is said that in one district the enumerators positively refused to note down any children under nine years of age. The number of foreigners in the country appears small when the large foreign trade is considered. The census puts them at 26,528, chiefly resident at Valparaiso, Santiago, and the mining capital of Copiapó. In reality, however, the foreign element in Chile is much larger than it would seem to be, for all children of foreigners born in the country are accounted Chileans. These and the offspring of mixed marriages together produce a considerable admixture of foreign blood not without its uses in so young a community. It is worthy of note, too, that these half-breeds, as a rule, show a marked attachment for the country of their birth. The census roughly estimated the independent Indian population at 44,000, 20,000 of whom are accredited to the debateable regions of Patagonia and Tierra del Fuego, and 24,000 to Araucania proper, but the last-mentioned figures are believed to be much below the mark.

Education.—The progress of late years in this direction has been not inconsiderable. In 1874 the Report of the Minister of Public Instruction showed that there were 806 public elementary schools, in which 62,244 scholars of both sexes were inscribed, and 478 private schools with 23,198 scholars. Altogether, therefore, there were throughout the Republic 1284 schools; in which 85,442 children were assumed to be receiving some degree of education; the proportion thus obtained being 1 scholar out of 4·94 children of educational age, or not far short of double what it was twenty years before. The cause of general education has received considerable attention at the hands of the Chilean Government in the last twenty-five years, but, from what Mr. Rumbold states, it would seem that it has been less warmly taken up by the more influential classes of the country. This, however,—it is only fair to add—finds some excuse in the stolid indifference of the peasantry, which is such as a severe compulsory system could probably alone overcome.

Post Office.—Closely allied to the development of education

are the postal statistics of the country, which may fitly claim a passing word. In 1852 71,168 letters, newspapers, and documents of all kinds passed through the post. In twelve years, that is in 1864, their number had risen to 4,375,408, while in 1874 it reached the figure 12,984,428, the increase in 1874 over the previous year being no less than 1,748,800. In 1874 there were transmitted 141,897 telegrams, for which 58,295 dollars were received, while, in 1875, the number rose to 161,459, and produced 59,866 dollars. The Transandine telegraph line of Messrs. Clark, which has been working for some years, is an achievement of which any country might well be proud; the wires are conveyed across the mountains on stone pillars, and in the more exposed portions carried underground.

Public Revenue and Expenditure.—The ordinary revenue for 1874 was £3,080,164, and the extraordinary revenue £52,180, or a total of £3,162,344, while the ordinary expenditure was £3,431,017, and the extraordinary expenditure £1,070,735, or a total of £4,501,772. The deficit on the year was therefore £1,369,428, of which £897,692 were taken out of the loan contracted in England in the previous year, and the remainder was met out of Treasury balances, and by a temporary advance from the National Bank of Chile. In this connection it may be remarked that the works of public utility undertaken in the last few years are of considerable magnitude, and though, for the most part, undoubtedly destined to be remunerative, the outlay incurred on them has sensibly disturbed the equilibrium of Chilean finance. For information respecting the Chilean loans, and the general indebtedness of the State, we must refer the reader to Mr. Rumbold's Report.

General Review of Exports.—Agricultural.—The Chilean exports, with the trifling exception of a few local manufactures and miscellaneous articles, altogether not amounting to one per cent of the whole, are made up of the mining produce and the agricultural produce (raw and manipulated) of the country in nearly equal proportions: but it is as a mining country that Chile is of absolute interest and importance to foreign markets.

A few notes with regard to the rise of Chilean agriculture will not be without interest. Up to the year 1848 Chile grew little beyond what she consumed, and the production of cereals was not looked upon as lucrative. The discovery of gold in California, in 1847, changed the whole aspect of things. The exports in wheat and flour alone increased nearly fourfold in two years, and sevenfold in seven. This

happy state of things could not, of course last for long, and from various causes the trade with California collapsed as suddenly as it had arisen. The exports to that country which in 1850 had reached 2,445,868 dollars (upwards of 1,500,000 in flour and wheat alone), and which made up nearly one-half of the entire exports for the year, had fallen to 275,763 dollars in 1855, and further fell to 178,484 dollars in 1858; the breadstuffs exported to San Francisco in the latter year only amounting to the insignificant sum of 15,000 dollars. But at the very time when the Californian "Eldorado" was eluding the grasp of the Chilean producers, a fresh but likewise transient outlet was opened to them by the Australian gold discoveries. This trade only lasted from 1853 to 1859, but in 1855 the exports represented no less a sum than 2,698,911 dollars, of which 2,541,692 dollars, or £508,000, was the value of the wheat and flour. Meanwhile equally remote and unforeseen causes were combining to assure to Chile a valuable and far steadier customer in Peru, which, in the last century sent wheat to Chile, and raised an abundance of grain, cattle, potatos, and other kinds of food, but had by degrees neglected her production, and taken to tropical husbandry, such as cotton and sugar-cane planting. The war in the United States for a time made cotton planting so lucrative as to turn Peruvian capital yet more in that direction. Thus Chile, which from the first year of which her statistics furnish any record, had supplied her neighbour with an average value of 250,000 dollars of breadstuffs, now found an outlet for five times that amount. The general conditions of the trade with Peru have been completely reversed in the last thirty years, as will be seen from the following figures:—

	1845. Dollars.	1863. Dollars.	1874. Dollars.
Imports from Peru . .	1,474,880	701,297	1,947,770
Exports to ,,	674,552	2,619,386	6,016,413

The development of the export trade with England has been even more surprising, for in 1874 the breadstuffs sent to this country had reached 6,457,945, the general exports in the same year figuring for 22,259,730 dollars.

Mineral Exports.—Passing over the trade in live stock, and a few agricultural productions of comparatively minor importance, we must devote a brief space to the mineral produce of the country. In the thirty years between 1844 and 1873 this amounted in value to the large sum of £73,888,018, from which, however, we ought to subtract

£4,082,195, the value of the gold and silver coin and guano exported, that of the latter amounting to about £105,000. The mineral exports doubled between 1844 and 1852 (rising from 3,618,987 dollars to 7,807,106), up to which last named year the mines of Atacama and Coquimbo had been but partially worked. In 1857 they had doubled again, and have since gone on increasing in value, with some variations, but not in the same rapid proportion. The mining province of Atacama, and, in particular, the district of Copiapo, from which nearly two-thirds of the Chilean mineral produce are derived, is said to show of late marked signs of exhaustion. In the most northern district of this province is situated the group of silver mines known as La Florida. These mines, which are of recent discovery, extend over a zone some twelve kilometres, and according to the latest accounts are of exceeding richness. At the great copper-smelting works at Guayacan the monthly out-turn of bar-copper exceeds £60,000 in value; and of nearly equal importance are the smelting works at Lota, south of Concepcion, nominally belonging to the Lota Coronel Company, but in reality the property of the Cousiño family. At Lota, too, the same family owns the largest coal mines as yet worked in Chile. Coal is found in many parts, especially in the southern coast districts, but unfortunately owing to its inferior quality, and to the heavy cost of extraction, the Chile coal must, for some years to come, compete at a great disadvantage with that brought from Great Britain at nominal freights by sailing vessels in quest of return cargoes. The value of the coal exported between 1844 and 1873 was about £1,064,200.

Concluding Remarks.—Mr. Rumbold not unfairly expresses a hope that the observations contained in his Report will convey the notion of a sober-minded, practical, laborious, well-ordered, and respectably-governed community, standing out in great contrast to the other States of kindred origin and similar institutions spread over the South-American continent. The blessings which Chile enjoys she owes to the pure traditions implanted in her administration by the founders of the Republic; to the preponderating share taken in public affairs by the higher and wealthier class; to the happy eradication of militarism; to the nearly entire absence of those accidental sources of wealth (gold, guano, and nitrate) so lavishly bestowed by Providence on some of her neighbours; to the consequent necessity for strenuous labour rapidly repaid by a bountiful soil; above all, perhaps,

to the neglect of her former masters, which, when she had cast off the yoke, drove her to create everything for herself, and called forth exceptional energies in the nation. Most of these may be summed up in two words—work and shrewd sense (*trabajo i cordura*).

Thanks largely to the natural advantages of their country, and not a little too to foreign—mostly English—energy and assistance, the Chilean people have now attained a remarkable degree of prosperity; but Mr. Rumbold points out that they have of late shown signs of the intoxicating effects of good fortune, and this is exhibited in the fact that they are bent (the Government and upper classes setting the example) rather on decorating and beautifying their house than on setting it in more perfect order. A first visit to the city of Santiago, he says, cannot but be matter of agreeable surprise to an intelligent European; but after a more lengthened stay the ambitious growth and luxury of the town will probably seem to him out of due proportion to the power and resources of the country of which it is the capital. One is, indeed, scarcely prepared to find 90 miles inland, at the foot of the Andes, a city of some 160,000 inhabitants, with such handsome public bulidings, stately dwelling-houses, and exceptionally fine promenades. On this subject the reader will find, in Mr. Rumbold's report, some further particulars of interest, to which want of space alone precludes us from referring in greater detail.

To conclude:—The opening years of the century were wont to see an anxious crowd watching at Valparaiso for the first sight of the ship which, at intervals during the year, was sent down from Peru to that port with supplies and luxuries commensurate with the wants of a needy outlying province. The same port saw close upon 3000 vessels —one-third of them steamers—entering and leaving it in 1874, while over 11,600 vessels traded in the year in all the harbours of the Republic. In the same year it was reckoned that the value of the capital in land throughout the country amounted to 666,000,000 dollars, or about £133,200,000, while the capital invested in banks, or in general commercial and industrial operations, might be safely estimated at over £30,000,000. Figures like these speak eloquently of the progress achieved in the three-quarters of a century that have elapsed since men gathered on the beach to wait the advent of the promised ship, and on the facts they reveal Chile might well rest a claim to universal regard and sympathy. But she can afford to rest that claim on far higher

grounds. She may fairly point to the pefectly pure, albeit somewhat dilatory, action of her courts of justice, the integrity of her administration, the general and willing obedience of her people to the law, and to the good sense of a community where men are content to busy themselves with their own concerns in preference to those of the State, and choose to live on the labour of their hands and brains rather than on the loaves and fishes of the public treasury. In these things Chile, adds Mr. Rumbold, stands nearly alone among South-American States, and accordingly should be classed apart from them.

NOTICES OF BOOKS.

Mesmerism, Spiritualism, &c., Historically and Scientifically Considered. Being Two Lectures delivered at the London Institution, with Preface and Appendix. By WILLIAM B. CARPENTER, C.B., M.D., F.R.S., &c., &c. London: Longmans, Green, and Co., 1877.

THE two lectures which Dr. Carpenter gave last year at the London Institution were generally reported by the press and led to some controversy. They were then published in Fraser's Magazine; and they are now re-published with what are considered to be *pièces justificatives* in an appendix. We may therefore fairly assume that the author has here said his best on the subject—that he has carefully considered his facts and his arguments—and that he can give, in his own opinion at least, good reasons for omitting to notice certain matters which seem essential to a fair and impartial review of the whole question.

Dr. Carpenter enjoys the great advantage, which he well knows how to profit by, of being on the popular side, and of having been long before the public as an expounder of popular and educational science. Everything he writes is widely read; and his reiterated assurances that nobody's opinion and nobody's evidence on this particular subject is of the least value unless they have had a certain *special early training* (of which, it is pretty generally understood, Dr. Carpenter is one of the few living representatives) have convinced many people that what he tells them must be true and should therefore settle the whole matter. He has another advantage in the immense extent and complexity of the subject and the widely scattered and controversial nature of its literature. By ranging over this wide field and picking here and there a fact to support his views and a statement to damage his opponents, Dr. Carpenter has rendered it almost impossible to answer him on every point, without an amount of detail and research that would be repulsive to ordinary readers. It is necessary therefore to confine ourselves to the more important questions, where the facts are tolerably accessible and the matter can be brought to a definite issue; though, if space permitted, there is hardly a page of the book in which we should not find expressions calling for strong animadversion, as, for example, the unfounded and totally false general assertion at p. 6, that "believers in spiritualism make it a reproach against men of science that they entertain a prepossession in favour of the ascertained and universally admitted laws of nature." Vague general assertions of this kind, without a particle of proof offered or which can be offered, are alone suffi-

cient to destroy the judicial or scientific claims of the work; but we have no intention of wasting space in further comment upon them.

Dr. Carpenter lays especial stress on his character of historian and man of science in relation to this enquiry. He parades this assumption in his title page and at the very commencement of his preface. He claims therefore to review the case as a judge, giving full weight to the evidence on both sides, and pronouncing an impartial and well-considered judgment. He may, indeed, believe that he has thus acted—for dominant ideas are very powerful—but any one tolerably acquainted with the literature and history of these subjects for the last thirty years, will most assuredly look upon this book as the work of an advocate rather than of a judge. In place of the impartial summary of the historian he will find the one-sided narrative of a partisan; and, instead of the careful weighing of fact and experiment characteristic of the man of science, he will find loose and inaccurate statements, and negative results set up as conclusive against positive evidence. We will now proceed to demonstrate the truth of this grave accusation, and shall in every case refer to the authorities by means of which our statements can be tested.

The first example of Dr. Carpenter's "historical" mode of treating his subject which we shall adduce, is his account (pp. 13 to 15) of the rise of mesmerism in this country owing to the successful performance of many surgical operations without pain during the mesmeric trance. Dr. Carpenter writes of this as not only an admitted fact, but (so far as any word in his pages shows), as a fact which was admitted from the first, and which never went through that ordeal of denial, misrepresentation, and abuse by medical men and physiologists that other phenomena are still undergoing from a similar class of men. Yet Dr. Carpenter was in the thick of the fight and must know all about it. He must know that the greatest surgical and physiological authorities of that day—Sir Benjamin Brodie and Dr. Marshall Hall—opposed it with all the weight of their influence, accused the patients of imposture, or asserted that they might be "naturally insensible to pain," and spoke of the experiments of Dr. Elliotson and others as "trumpery," and as "polluting the temple of science." He must know, too, that Dr. Marshall Hall professed to demonstrate "physiologically" that the patients were impostors, because certain reflex-actions of the limbs which he declared ought to have occurred during the operations did not occur. The medical periodicals of the day were full of this, and a good summary may be found in Dr. Elliotson's "Surgical Operations without Pain, &c.," London, 1843. Dr. Carpenter tells us how his friends, Dr. Noble and Sir John Forbes, in 1845 accepted and wrote in favour of the reality of the facts; but it was hardly "historical" to tell us this as the whole truth, when, for several years previously, the most violent controversy, abuse, and even

persecution, had raged on this very matter. Great physiological authorities were egregiously in the wrong then, and the natural inference to those who know the facts is, that other physiological authorities who now deny equally well attested facts may be no more infallible than their predecessors.

Dr. Carpenter persistently denies that there is any adequate evidence of the personal influence of the mesmeriser on the patient independent of the patient's knowledge and expectation, and he believes himself to be very strong in the cases he adduces, in which this power has been tested and failed. But he quite ignores the fact that all who have ever investigated the higher phases of mesmerism—such as influence at a distance, community of sensation, transference of the senses, or true clairvoyance—agree in maintaining that these phenomena are very uncertain, depending greatly on the state of body and mind of the patient, who is exceedingly susceptible to mental impressions, the presence of strangers, fatigue, or any unusual conditions. Failures continually occur, even when the mesmeriser and patient are alone or when only intimate friends are present; how, then, can the negative fact of a failure before strangers and antagonists prove anything? Dr. Carpenter also occupies his readers' attention with accounts of hearsay stories which have turned out exaggerated or incorrect, and lays great stress on the "disposition to overlook sources of fallacy," and to be "imposed on by cunning cheats" which this shows. This may be admitted; but it evidently has no bearing on well-authenticated and carefully observed facts, perfectly known to every student of the subject. Our author maintains, however, that such facts do not exist, and that "the evidence for these higher marvels has invariably broken down when submitted to the searching tests of trained experts." Here the question arises, who are "trained experts?" Dr. Carpenter would maintain that only sceptical medical men and professed conjurors deserve that epithet, however ignorant they may be of all the conditions requisite for the study of these delicate and fluctuating phenomena of the nervous system. But we, on the contrary, would only give that name to enquirers who have experimented for months or years on this very subject, and are thoroughly acquainted with all its difficulties. When such men are also physiologists it is hardly consistent with the historical and scientific method of enquiry to pass their evidence by in silence. I have already called Dr. Carpenter's attention to the case of the lady residing in Professor Gregory's own house, who was mesmerised at several streets distance by Mr. Lewis without her knowledge or expectation. This is a piece of direct evidence of a very satisfactory kind, and outweighs a very large quantity of negative evidence; but no mention is made of it except the following utterly unjustifiable remark:—"His (Mr. Lewis's) utter failure under the scrutiny of sceptical enquirers, obviously discredits all his previous statements, except

to such as (like Mr. A. R. Wallace, who has recently expressed his full faith in Mr. Lewis's self asserted powers) are ready to accept without question the slenderest evidence of the greatest marvels." ("Mesmerism, Spiritualism, &c.," p. 24.) Now will it be believed that this statement, that I "place full faith in Mr. Lewis's *self-asserted powers*," has not even the shadow of a foundation. I know nothing of Mr. Lewis or of his powers, self-asserted or otherwise, but what I gain from Prof. Gregory's account of them; and in my letter to the "Daily News," immediately after the delivery of Dr. Carpenter's lectures, I referred to this account. I certainly have "full faith" in Professor Gregory's very careful narrative of a fact entirely within his own knowledge. This may be "the slenderest evidence" to Dr. Carpenter, but slender or not he chooses to evade it, and endeavours to make the public believe that I and others accept the unsupported assertions of an unknown man. It is impossible adequately to characterise such reckless accusations as this without using language which I should not wish to use. Let us pass on, therefore, to the evidence which Dr. Carpenter declares to be fitly described as "the slenderest." M. Dupotet, at the Hotel de Dieu, in Paris, put a patient to sleep when behind a partition, in the presence of M. Husson and M. Recamier, the latter a complete sceptic. M. Recamier expressed a doubt that the circumstances might produce expectation in the patient, and himself proposed an experiment the next day, in which all the same conditions should be observed, except that M. Dupotet should not come at all till half an hour later. He anticipated that the "expectation" would be still stronger the second time than at first, and that the patient would be mesmerised. But the result was quite the reverse. Notwithstanding every minute detail was repeated as on the previous day when the operator was in the next room, the patient showed no signs whatever of sleep either natural or somnambulic (Teste's "Animal Magnetism," Spillan's Translation, p. 159). The Commission appointed by the Academie Royale de Medicine in 1826 sat for five years and investigated the whole subject of animal magnetism. It was wholly composed of medical men, and in their elaborate report, after giving numerous cases, the following is one of their conclusions:—

"14. We are satisfied that it (magnetic sleep) has been excited under circumstances where those magnetised could not see, and were entirely ignorant of the means employed to occasion it."

These were surely "trained experts;" yet they declare themselves satisfied of that, the evidence for which, Dr. Carpenter says, has always broken down when tested.

Baron Reichenbach's researches are next discussed, and are coolly dismissed with the remark that "it at once became apparent to experienced physicians, that the whole phenomena were subjective, and that 'sensitives' like Von Reichenbach's can

feel, see, or smell anything they were led to believe they *would* feel, see, or smell." His evidence for this is, that Mr. Braid could make his subjects do so, and that Dr. Carpenter had seen him do it. One of them, for instance,—an intellectual and able Manchester gentleman,—"could be brought to see flames issuing from the poles of a magnet of any form or colour that Mr. Braid chose to name." All this belongs to the mere rudiments of mesmerism and is known to every operator. Two things, however, are essential—the patient or sensitive must be, or have been, mesmerised, or electro-biologised as it is commonly called, and the *suggestion* must be actually made. Given these two conditions and no doubt twenty persons may be made to declare that they see green flames issuing from the operator's mouth; but no single case has been adduced of persons in ordinary health, not subject to any operation of mesmerism, &c., being all caused to see this or any other thing in agreement, by being merely brought into a dark room and asked to describe accurately what they saw. Yet this is what Von Reichenbach did, and much more. For, in order to confirm the evidence of the "sensitives" first experimented on, he invited a large number of his friends and other persons in Vienna to come to his dark room, and the result was that about *sixty persons* of various ages and conditions saw and described exactly the same phenomena. Among these were a number of literary, official, and scientific men and their families, persons of a status fully equal to that of Dr. Carpenter and the Fellows of the Royal Society—such as Dr. Nied, a physician; Professor Endlicher, director of the Imperial Botanic Garden; Chevalier Hubert von Rainer, barrister; Mr. Karl Schuh, physicist; Dr. Ragsky, Professor of Chemistry; Mr. Franz Kollar and Dr. Diesing, Curators in the Imperial Natural History Museum, and many others. There was also an artist, Mr. Gustav Anschütz, who could see the flames, and drew them in their various forms and combinations. Does Dr. Carpenter really ask his readers to believe that his explanation applies to these gentlemen? That they all quietly submitted to be told *what* they were to see, submissively *said* they saw it, and allowed the fact to be published at the time, without a word of protest on their part from that day to this? But a little examination of the reports of their evidence shows that they did not follow each other like a flock of sheep, but that each had an individuality of perceptive power, some seeing one kind of flame better than another; while the variety of combinations of magnets submitted to them, rendered anything like suggestion as to what they were to see quite impossible, unless it were a deliberate and wilful imposture on the part of Baron von Reichenbach.

But again, Dr. Carpenter objects to the want of tests, and especially his pet test of using an electro-magnet, and not letting the patients know whether the electric circuit which "makes" and "unmakes" the magnet was complete or broken. How

far this test, had it been applied, would have satisfied the objector, may be imagined from his entirely ignoring all the tests, many of them at least as good, which were actually applied. The following are a few of these:—Test 1. Von Reichenbach arranged with a friend to stand in another room with a stone wall between him and the patient's bed, holding a powerful magnet, the armature of which was to be closed or opened at a given signal. The patient detected, on every occasion, whether the magnet was opened or closed. Test 2. M. Baumgartner, a professor of physics, after seeing the effects of magnets on patients, took from his pocket what he said was one of his most powerful magnets, to try its effects. The patient, to Von Reichenbach's astonishment, declared she found this magnet on the contrary very weak, and its action on her hardly more perceptible than a piece of iron. M. Baumgartner then explained that this magnet, though originally very powerful, had been as completely as possible deprived of its magnetism, and that he had brought it as a test. Here was *suggestion* and *expectation* in full force, yet it did not in the least affect the patient. (For these two tests see "Ashburner's Translation of Reichenbach," pp. 39, 40.) Test 3. A large crystal (placed in a new position before each patient was brought into the dark room) was always at once detected by means of its light, yellower and redder than that from magnets (*loc. cit.*, p. 86). Test 4. A patient confined in a darkened passage held a wire which communicated with a room in which experiments were made on plates connected with this wire. As these plates were exposed to sunlight or shade, the patient described corresponding changes in the luminous appearances of the end of the wire (*loc. cit.* p. 147). Test 5. The light from magnets, &c., was thrown on a screen by a lens, so that the image could be instantly and noislessly changed in size and position at pleasure. Twelve patients, eight of them healthy and new to the enquiry, saw the image, and described its alterations of size and position as the lens or screen was shifted in the dark (*loc. cit.*, p. 585). Dr. Carpenter's only reply to all this is, that "Baron Reichenbach's researches upon 'Odyle' were discredited a quarter of a century ago, alike by the united voice of scientific opinion in his own country, and by that of the medical profession here." Even if this were the fact, it would have nothing to do with the matter, which is one of experiment and evidence, not of the belief or disbelief of certain prejudiced persons, since to *discredit* is not to *disprove*. The painless operations in mesmeric sleep were "discredited" by the highest medical authorities in this country, and yet they were true. But Dr. Elliotson, Dr. Ashburner, and others, accepted Reichenbach's discoveries; and some of the Vienna physicians even, after seeing the experiments with persons "whose honour, truthfulness, and impartiality they could vouch for," also accepted them as proved.

The facts of the luminosity of magnets was also independently established by Dr. Charpignon, who, in his "Physiologie, Médicine, et Metaphysique du Magnetisme," published in 1845—the very same year in which the account of Von Reichenbach's observations first appeared—says: "Having placed before the sonnambulists four small bars of iron, one of which was magnetised by the loadstone, they could always distinguish this one from the others, from its two ends being enveloped in a brilliant vapour. The light was more brilliant at one end (the north pole) than at the other. I could never deceive them; they always recognised the nature of the poles, although when in their normal state they were in complete ignorance of the subject." Surely here is a wonderful confirmation. One observer in France and another in Germany make the same observation about the same time, and quite independently; and even the detail of the north pole being the more brilliant agrees with the statement of Reichenbach's sensitives (Ashburner's Trans., p. 20).

Our readers can now judge how far the historic and scientific method has been followed in Dr. Carpenter's treatment of the researches of Von Reichenbach, not one of the essential facts here stated (and there are hundreds like them) being so much as alluded to, while "suggestion," expectation," and "imposture," are offered as fully explaining everything. We cannot devote much time to the less important branches of the subject, but it is necessary to show that *in every case* Dr. Carpenter misstates facts and sets negative above positive evidence. Thus, as to the magnenometer* and odometer of Mr. Rutter and Dr. Mayo, all the effects are imputed to expectation and unconscious muscular action, and we have this positive statement: "It was found that the constancy of the vibrations depended entirely upon the operator's watching their direction, and, further, that when such a change was made *without the operator's knowledge* in the conditions of the experiment, as *ought*, theoretically, to alter the direction of the oscillations, no such alteration took place." Yet Mr. Rutter clearly states—1. That the instrument can be affected through the hand of a *third* person with exactly the same result (Rutter's "Human Electricity," App., p. 54). 2. That the instrument is affected by a crystal on a *detached stand* brought close to the instrument, but without contact (*loc. cit.*, p. 151). 3. That many persons, however "expectant" and anxious to succeed, have no power to move the instrument. 4. That substances *unknown to the operator*, and even when held by a third party caused correct indications, and that an attempt to deceive by using a substance under a wrong name was detected by the movements of the instrument (*loc. cit.*, Appendix, p. lvi.). Here then Mr. Rutter's posi-

* The magnenometer is a delicate pendulum which, when its support is touched by certain persons, vibrates in a definite direction, the direction changing on the motion suddenly stopping when different substances are touched at the same time by the operator.

tive testimony is altogether ignored, while the negative results of another person are set forth as conclusive. Next we have the evidence for the divining-rod similarly treated. Dr. Mayo is quoted as supporting the view that the rod moved in accordance with the "expectations" of the operator, but on the preceding page of Dr. Mayo's work, other cases are given in which there was no expectation; and the fact that Dr. Mayo was well aware of this source of error, and was a physiologist and physician of high rank, entitles his opinion as to the reality of the action in other cases to great weight. Again, we have the testimony of Dr. Hutton, who saw the Hon. Lady Milbanke use the divining-rod on Woolwich Common, and who declares that it turned where he knew there was water, and that in other places where he believed there was none it did not turn: that the lady's hands were closely watched, and that no motion of the fingers or hands could be detected, yet the rod turned so strongly and persistently that it became broken. No other person present could voluntarily or involuntarily cause the rod to turn in a similar way (Hutton's "Mathematical Recreations," Ed. 1840, p. 711). The evidence on this subject is most voluminous, but we have adduced sufficient to show that Dr. Carpenter's supposed demonstration does not account for all the facts.

We now come to the very interesting and important subject of clairvoyance, which Dr. Carpenter introduces with a great deal of irrelevant matter calculated to prejudge the question. Thus, he tells his readers that "there are at the present time numbers of educated men and women who have so completely surrendered their 'common sense' to a dominant prepossession as to maintain that any such monstrous fiction (as of a person being carried through the air in an hour from Edinburgh to London) ouught to be believed, even upon the evidence of a single witness, if that witness be one upon whose testimony we should rely in the ordinary affairs of life!" He offers no proof of this statement, and we venture to say he can offer none, and it is only another example of that complete misrepresentation of the opinions of his opponents with which this book abounds. At page 71, however, we enter upon the subject itself, and at once encounter one of those curious examples of ignorance (or suppression of evidence) for which Dr. Carpenter is so remarkable in his treatment of this subject. We have been already told (p. 11) of the French Scientific Commission which about a hundred years ago investigated the pretensions of Mesmer, and decided, as might have been anticipated, against him. Now, we have the statement that "it was by the French Academy of Medicine, in which the mesmeric state had been previously discussed with reference to the performance of surgical operations, that this new and more extraordinary claim (*clairvoyance*) was first carefully sifted, in consequence of the offer made in 1837 by M. Burdin of a prize of 3000 francs to anyone who should

be found capable of reading through opaque substances." The result was negative. No clairvoyant succeeded under the conditions imposed. The reader unaccustomed to Dr. Carpenter's historical method would naturally suppose this statement to be correct, and that *clairvoyance* was *first* carefully sifted in France *after* 1837, though he might well doubt, if offering a prize for reading under rigid conditions was an adequate means of sifting a faculty so eminently variable, uncertain, and delicate as clairvoyance is admitted to be. What, then, will be his astonishment to find that this same "Académie Royal de Medicine" had appointed a commission of eleven members in 1826, who inquired into the whole subject of mesmerism for *five years*, and in 1831 reported in full, and *in favour* of the reality of almost all the alleged phenomena, *including clairvoyance*. Of the eleven members, nine attended the meetings and experiments, and all nine signed the report, which was therefore unanimous. This report, being full and elaborate, and the result of personal examination and experiment by medical men—the very "trained and sceptical experts," who are maintained by Dr. Carpenter to be the only adequate judges—is wholly ignored by him. In this report we find among the conclusions—"24. We have seen two sonnambulists distinguish with their eyes shut objects placed before them: name cards, read books, writing, &c. This phenomenon took place even when the opening of the eyelids was accurately closed by means of the fingers."* Is it not strange that the "historian" of mesmerism, &c., should be totally ignorant of the existence of this report, which is referred to in almost every work on the subject? Yet he must be thus ignorant or he could never say, as he does in the very same page quoted above (p. 71), "that in every instance (so far as I am aware) in which a thorough investigation has been made into those 'higher phenomena' of mesmerism, the supposed proof has completely failed." It cannot be said that investigation by nine medical men carried on for five years with every means of observation and experiment, and elaborately reported on, was not "thorough," whence it follows that Dr. Carpenter must be ignorant of it, and our readers can draw their own inference as to the value of his opinion, and the dependence to be placed on his scientific and and historical treatment of this subject.

More than twenty-five pages of the book are occupied with more or less detailed accounts of the failures and alleged exposures of clairvoyants, while not a single case is given of a clairvoyant having stood the test of rigid examination by a committee, or by medical or other experts, and the implication is that none such are to be found. But every enquirer knows that clairvoyance is a most delicate and uncertain phenomenon, never to be certainly calculated on, and this is repeatedly stated in the

* Archives Generales de Medecine, vol. xx.; also in LEE's Animal Magnetism, pp. 13 to 29.

works of Lee, Gregory, Teste, Deleuze, and others. How, then, can any number of individual failures affect the question of the reality of the comparatively rare successes. As well deny that any rifleman ever hit the bull's-eye at 1000 yards, because none can be sure of hitting it always, and at a moment's notice. Several pages are devoted to the failure of Alexis and Adolphe Didier under test conditions in England, ending with the sneering remark, "Nothing, so far as I am aware, has ever been since heard of this *par nobile fratrum*." Would it (to use an established formula) surprise Dr. Carpenter to hear that these gentlemen remained in England a considerable time after the date he alludes to, that they have ever since retained their power and reputation, and that both still practise successfully medical clairvoyance, the one in London, and the other in Paris? To balance the few cases of failure by Alexis, Dr. Lee has given his personal observations of ten times as many successes, some of them of the most startling kind ("Animal Magnetism," pp. 255, 277). We can only find room here for two independent and complete tests. The first is given by Serjeant Cox as witnessed by himself. A party of experts was planned to test Alexis. A word was written by a friend in a distant town and enclosed in an envelope, *without any of the party knowing what the word was*. This envelope was enclosed successively in six others of thick brown paper, each sealed. This packet was handed to Alexis, who placed it on his forehead, and in three minutes and a half wrote the contents correctly, imitating the very handwriting. ("What am I," vol. ii., p. 167.) Now unless this statement by Serjeant Cox is absolutely false, a thousand failures cannot outweigh it. But we have, if possible, better evidence than this; and Dr. Carpenter knows it, because I called his attention to it in the "Daily News." Yet he makes no allusion to it. I refer to the testimony of Robert Houdin, the greatest of modern conjurers, whose exploits *are quoted* by Dr. Carpenter, when they serve his purpose (pp. 76, 111). He was an absolute master of card-tricks, and knew all their possibilities. He was asked by the Marquis de Mirville to visit Alexis, which he did twice. He took his own new cards, dealt them himself, but Alexis named them as they lay on the table, and even named the trump before it was turned up. This was repeated several times, and Houdin declared that neither chance nor skill could produce such wonderful results. He then took a book from his pocket and asked Alexis to read something eight pages beyond where it was opened at a specified level. Alexis pricked the place with a pin, and read four words, which were found at the place pricked nine pages on. He then told Houdin numerous details as to his son, in some of which Houdin had tried to deceive him, but in vain; and when it was over Houdin declared it "stupefying," and the next day signed a declaration that the report of what took place was correct, adding, "the

more I reflect upon them the more impossible do I find it to class them among the tricks which are the object of my art." The two letters of Robert Houdin were published at the time (May, 1847) in "Le Siècle," and have since appeared in many works, among others in Dr. Lee's "Animal Magnetism" (pp. 163 and 231).

One of the supposed exposures made much of by Dr. Carpenter is that of Dr. Hewes's "Jack," which is suggestive as showing the complete ignorance of many experimenters thirty years ago as to the essential conditions of the manifestation of so delicate and abnormal a faculty as clairvoyance, ignorance shared in by believers and sceptics alike. According to Dr. Carpenter (whose account he informs me is taken from an article by Dr. Noble in the "British and Foreign Medical Review" of April, 1845), Jack's eyes were "bound down by surgeons with strips of adhesive plaster, over which were folds of leather, again kept in place by other plasters." Jack then read off, *without the least hesitation*, everything that was presented to him. But a young Manchester surgeon had his eyes done up in the same manner, and, by *working the muscles of his face* till he had *loosened the plasters*, was enabled to read by *looking upwards*. The conclusion was immediately jumped at that this was the way Jack did it, although no *working of the muscles of the face* had been observed, and no *looking upwards* described. Instead, however, of repeating the experiment under the same conditions, but more watchfully, it was proposed that the *entire eyes should be covered up with a thick coating of shoemakers' wax!* The boy objected and resisted, and it was put on by force; and then, the clairvoyant powers being annihilated, as might have been anticipated, there was great glorification among the sceptics, and Dr. Carpenter indulges himself in a joke, telling us that Jack now "plainly saw, even with his eyes shut, that his little game was up." To any one who considers this case, even as related by Dr. Carpenter, it will be evident that the boy was a genuine clairvoyant. Adhesive plaster properly applied by a medical man on a passive subject, is not to be loosened by imperceptible working of the muscles, and it is too great a demand upon our credulity to ask us to believe that this occurred undetected by the acute medical sceptics watching the whole procedure. We have, however, fortunately, another case to refer to, in which this very test was carried out to its proper conclusion by examining the state of the plaster *after the clairvoyance*, when the alleged looseness could be instantly detected. A clairvoyant boy at Plymouth was submitted to the examination of a sceptical committee, who appear to have done their work very thoroughly. First his eyes were examined, and it was found that the balls were so turned up that even were the eyelids a little apart, ordinary vision was impos-

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sible.* Then he was closely watched, and while the eyelids were seen to be perfectly closed, he read easily. Then adhesive plaster was applied, carefully warmed, in three layers, and it was watched to see that the adhesion was perfect all round the edges. Again the boy read what was presented to him, sometimes easily, sometimes with difficulty. At the end of the experiments the plaster was taken off strip by strip by the committee, and it was found to be perfectly secure, and the eyelids so completely glued together that it was a work of some difficulty to get them open again. This case is recorded, with the names of the committee in the "Zoist," vol. iv., pp. 84—88; and I call the reader's attention to the *completeness* of the test here, and its demonstration of the reality of clairvoyance, as compared with the loose experiment and hasty jumping-to-a-conclusion in the case which Dr. Carpenter thinks alone worthy of record.

Dr. Carpenter next comes to the work of Professor Gregory ("Letters on Animal Magnetism,") and devotes several pages to assertions as to the professor's "credulity," the "reprehensible facility" with which he accepted Major Buckley's statements, the "entire absence of detail" as to "precautions against tricks," and his utter failure to find a clairvoyant to obtain Sir James Simpson's bank-note. "And yet," he says, referring especially to myself, "there are even now, men of high scientific distinction, who adduce Professor Gregory's testimony on this subject as unimpeachable!" Readers who have accompanied me so far, will at least hestitate to accept Dr. Carpenter's dictum on this point, till they have heard what can be said on the other side. To give full details would occupy far too much space, I must therefore refer my readers to Professor Gregory's book for some cases, and give merely a brief outline of others. At page 394 (Case 29) is given in detail a most remarkable test-case, in which Professor Gregory sent some handwriting from Edinburgh to Dr. Haddock's clairvoyant at Bolton; who gave in return a minute description of the writer, her appearance, dress, house, illness, medical treatment, &c. At page 401 another test of the same kind is described. At page 403 a number of such cases are summarised, and one very completely given in detail. At page 423 is an account of a clairvoyant boy at the house of Dr. Schmitz, Rector of the High School at Edinburgh. This boy described Professor Gregory's house accurately, and the persons at that time in the dining-room (afterwards ascertained to be correct). As a further test Dr. Schmitz was asked to go into another room with his son and do anything he liked. The boy then described their motions, their jumping about, the son going out and coming in again, and the doctor beating his son with a roll of paper. When Dr. Schmitz returned, Professor Gregory repeated all the boy had said, which the doctor, much astonished,

* This is a constant feature of the true mesmeric trance, but "Jack's" accusers seem to have known nothing about it.

declared to be correct in every particular. At page 445 (Case 42(is an account of another clairvoyant, a mechanic, who described Professor Gregory's house in detail, and saw a lady sitting in a particular chair in the drawing-room reading a new book. On returning home the professor found that Mrs. Gregory had, at the time been sitting in that particular chair, which she hardly ever was accustomed to use, and was reading a new book which had been sent to her just before, but of which the Professor knew nothing. At page 405 is a most remarkable case of the recovery of a stolen watch, and detection of the thief in London by Dr. Haddock's clairvoyant at Bolton. The letters all passed through Sir Walter C. Trevelyan, who showed them to Professor Gregory. At page 407 are the particulars of the extraordinary discovery of the locality of travellers by means of their handwriting only, sent from the Royal Geographical Society to Sir C. Trevelyan in Edinburgh, and by him to Bolton, he himself not knowing either the names of the travellers, or where they were. Many more cases might be referred to, but these are sufficient to show that there is not that "total absence of detail," and of "precautions," in Professor Gregory's experiments, which is Dr. Carpenter's reason for entirely ignoring them. In addition to this we have the account of Dr. J. Haddock, a physician practising at Bolton, of the girl Emma, who for nearly two years was under his care, and residing in his house. Many of Professor Gregory's experiments, and those of Sir Walter Trevelyan, were made through this girl, and a full account of her wonderful clairvoyant powers is given by Dr. Haddock in the Appendix to his "Somnolysm and Psycheism." She could not read, and did not even know her letters. The discovery of the stolen cashbox, and identification of the entirely unsuspected thief, is given in full by Dr. Haddock, and is summarised in my "Miracles and Modern Science," p. 64. Again, Dr. Herbert Mayo gives unexceptionable personal testimony to clairvoyance at pages 167, 172, and 178 of his book on "Popular Superstitions."

Dr. Carpenter is very severe on Professor Gregory for his belief in Major Buckley's clairvoyants reading mottoes in nuts, &c., but Major Buckley was a man of fortune and good position, who exercised his remarkable powers as a magnetiser for the interest of it, and there is not the slightest grounds for suggesting his untrustworthiness. We have beside the confirmatory testimony of other persons, among them of Dr. Ashburner, who frequently took nuts purchased by himself, and had them correctly read by the clairvoyants before they were opened. ("Ashburner's Philosophy of Animal Magnetism," p. 304.) Dr. Carpenter also doubts Professor Gregory's common sense in believing that a sealed letter had been read unopened by a clairvoyant when it might have been opened and resealed; but he omits to say that the envelopes were expressly arranged to pre-

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vent their being opened without detection, and that the Professor adds, "I have in my possession one of the envelopes thus read, which has since been opened, and I am convinced that the precautions taken precluded any other than lucid vision."*

Still more important, perhaps, is the testimony of many eminent physicians to the existence of these remarkable powers. Dr. Rostan, Parisian Professor of Medicine, in his article "Magnetisme," in the "Dictionnaire de Medecine," says (as quoted by Dr. Lee), "There are few things better demonstrated than clairvoyance. I placed my watch at a distance of three or four inches from the occiput of the sonnambulist, and asked her if she saw anything. 'Certainly,' she replied, 'it is a watch; ten minutes to eight.' M. Ferrus repeated the experiment with the same successful result. He turned the hands of his watch several times, and we presented it to her *without looking at it;* she was not once mistaken." The Commissioners of the Royal Académie de Medecine applied the excellent test of holding a finger on each eye-lid, when the clairvoyant still read the title of a book, and distinguished cards. (Quoted in Dr. Lee's "Animal Magnetism," p. 22.) Dr. Esdaile had a patient at Calcutta who could hear and see through the stomach. This was tested by himself with a watch, as in the French case quoted above. ("Zoist," vol. viii., p. 220.) Dr. Teste's account of the clairvoyance of Madame Hortense is very suggestive. She sometimes read with ease when completely bandaged, and when a paper was held between her eyes and the object; at other times she could see nothing, and the smallest fatigue or excitement caused this difference. This excessive delicacy of the conditions for successful clairvoyance render all public exhibitions unsatisfactory; and Professor Gregory "protests against the notion that it is to be judged by the rough experiments of the public platform, or by such tests as can be publickly applied." For the same reason direct money tests are always objected to by experienced magnetisers, the excitement produced by the knowledge of the stake or the importance of the particular test impairing or destroying the lucidity. This is the reason why gentlemen and physicians like Professor Gregory, Major Buckley, and Dr. Haddock, who have had the command of clairvoyants, have not attempted to gain the bank-notes which have at various times been offered. Dr. Carpenter was very irate because I suggested at Glasgow—not as he seems to have understood that there *was* no note in Sir James Simpson's envelope—but that the clairvoyants themselves, if they heard of it, might very

* Dr. Carpenter says that "the unsealing of letters and the re-sealing them so as to conceal their having been opened" are practised in Continental post-offices. No doubt this can be done with an ordinary letter, but it is no less certain that there are many ways of securing a letter which absolutely preclude its being done undetected, and Dr. Carpenter omits to state that such precautions are here expressly mentioned by Professor Gregory as having been used in these experiments.

well be excused if they thought it was a trick to impose upon them. I find now that in the other case quoted by Dr. Carpenter, the note for £100 publicly stated to have been enclosed by Sir Philip Crampton in a letter, and placed in a bank in Dublin, to become the property of any clairvoyant who should read the *whole of it*—this was actually the case. After six months the letter was opened, and the manager of the bank certified that it contained no note at all, but a blank cheque! The correspondence on the subject is published in the "Zoist," vol. x., p. 35. Dr. Carpenter's indignation was therefore misplaced; for, as a medical knight in Ireland did actually play such a trick, the mere supposition on my part, that ignorant clairvoyants might think that a medical knight in Scotland was capable of doing the same, was not a very outrageous one.

We now come to the last part of Dr. Carpenter's lecture—Table-Turning and Spiritualism, and here there is hardly any attempt to deal with the evidence. Instead of this we have irrelevant matters put prominently forward, backed up by sneers against believers, and false or unproved accusations against mediums. To begin with, the old amusement of table-turning of fifteen or twenty years ago, with Faraday's proof that it was often caused by unconscious muscular action, is again brought to the front. Table-tilting is asserted to be caused in the same way, and an "indicator" is suggested for proving this; and the whole matter is supposed to be settled because no one, so far as Dr. Carpenter is aware, "has ever ventured to affirm that he has thus demonstrated the *absence* of muscular pressure," and "until such demonstrations shall have been given, the tilting—like the turning—of tables may be unhesitatingly attributed to the unconscious muscular action of the operators." We suppose Dr. Carpenter will shield himself by the "thus" in the above sentence, though he knows very well that a far more complete demonstration of the absence of muscular pressure than any indicator could afford has been repeatedly given, by motion, both turning and tilting, of the table occurring *without any contact whatever*. Thus, in the Report of the Committee of the Dialectical Society, we have (p. 378), Experiment 13, nine members present, all stood quite clear of the table, and observers were placed under it to see that it was not touched, yet it repeatedly moved along the floor, often in the direction asked for. It also jerked up from the floor about an inch. This was repeated when all stood 2 feet from the table. Experiment 22. Six members present, the same thing occurred under varied conditions. Experiment 38 (p. 390). Eight members present, the conditions were most rigid; the chairs were all turned with their backs to the table at a foot distant from it; every member present knelt on his chair *with his hands behind his back*; there was abundance of light, yet, under these test-conditions, the table moved several times in various directions, visible to all present. Finally

the table was turned up and examined, and found to be an ordinary dining table with no machinery or apparatus of any kind connected with it. Similar movements without contact have been witnessed elsewhere and recorded by Serjeant Cox and by Mr. Crookes, as well as by many other persons; yet the man who comes before the public as the "historian" of this subject tells his audience and his readers that "he is not aware that anyone affirms that he has demonstrated the absence of muscular pressure"! How are we to reconcile this statement with Dr. Carpenter's references to each of the books, papers, or letters containing the facts above quoted or referred to? But we have evidence of a yet more conclusive character (from Dr. Carpenter's own point of view), because it is that of a medical man who has made a special study of abnormal mental phenomena. Dr. Lockhart Robertson, for many years an editor of the "Journal of Mental Science" and Superintendent of the Hayward's Heath Asylum, declares that his own heavy oak dining table was lifted up and moved about the room, and this not by any of the four persons present. Writing was also produced on blank paper which the medium "had not the slightest chance of touching" ("Dialectical Report," p. 248). Dr. Carpenter is always crying out for "sceptical experts," but when they come—in the persons of Robert Houdin and Dr. Lockhart Robertson, he takes very good care that, so far as he is concerned, the public shall not know of their existence. What, therefore, is the use of his asking me (in a note at p. 108) whether my table ever went up within its crinoline in the presence of a "sceptical expert"? The very fact that I *secretly* applied tests (see "Miracles and Modern Spiritualism," p. 134) shows that I was myself sceptical at this time, and several of my friends who witnessed the experiments were far more sceptical, but they were all satisfied of the completeness of the test. The reason why some sceptical men of science never witness these successful experiments is simply because they will not persevere. Neither Dr. Carpenter nor Professor Tyndall would come more than once to my house to see the medium through whom these phenomena occurred, or I feel sure they might, after two or three sittings, have witnessed similar phenomena themselves. This has rendered all that Dr. Carpenter has seen at odd times during so many years of little avail. He has had one, or at most two, sittings with a medium, and has taken the results, usually weak or negative, as proving imposture, and then has gone no more. Quite recently this has happened with Dr. Slade and Mrs. Kane; and yet this mode of enquiry is set up as against that of men who hold scores of sittings for months together with the same medium, and after guarding against every possibility of deception or delusion obtain results which seem to Dr. Carpenter incredible. Mr. Crookes had a long series of sittings with Miss Kate Fox (now Mrs. Jencken) in his own

house, and tested the phenomena in every way his ingenuity could devise. Dr. Carpenter was recently offered the same facilities with this lady and her sister, but as usual had only one sitting. Yet he thinks it fair and courteous to make direct accusations of imposture against both these ladies. He revives the absurd and utterly insufficient theory that the "raps" are produced by "a jerking or snapping action of particular tendons of either the ankles, knees, or toes." The utter childishness of this explanation is manifest to any one who has heard the sounds through any good medium. They vary from delicate tickings to noises like thumpings with the fist, slappings with the hand, and blows with a hammer. They are often heard loudly on the ceiling or on a carpetted floor, and heard as well as felt on the backs or seats of chairs quite out of reach of the medium. One of the sceptical committees in America tested the Misses Fox by placing them barefooted on pillows, when the "raps" were heard as distinctly as before on the floor and walls of the room. Mr. Crookes states that he has heard them on the floor, walls, &c., when Miss Fox was suspended in a swing from the ceiling, and has felt them on his own shoulder. He has also heard them on a sheet of paper suspended from one corner by a thread held between the medium's fingers. A similar experiment was tried successfully by the Dialectical Committee ("Report," p. 383). At a meeting of the same committee raps were heard on a book while in the pocket of a very sceptical member; the book was placed on the table, and raps again heard; it was then held by two members supported on ivory paper knives, when still raps were heard upon it ("Report," p. 386).

Again, there is the evidence of Professor Barrett, an experienced physicist, who entered on this enquiry a complete sceptic. He tells us that he examined the raps or knockings occurring in the presence of a child ten years of age—that in full sunlight, when every precaution to prevent deception had been taken—still the raps would occur in different parts of the room, entirely out of reach of the child, whose hands and feet were sometimes closely watched, at other times held. The phenomena have been tested in every way that the ingenuity of sceptical friends could devise; and as Professor Barrett is well acquainted with Dr. Carpenter's writings on the subject and the explanations he gives, we have here another proof of the utter worthlessness of these explanations in presence of the facts themselves.

The Honourable R. D. Owen has heard, in the presence of Miss Fox, blows as if made by a strong man using a heavy bludgeon with all his force, blows such as would have killed a man or broken an ordinary table to pieces; while on another occasion the sounds resembled what would be produced by a falling cannon-ball, and shook the house ("Debateable Land," p. 275); and Dr. Carpenter would really have us believe that all these wonderfully varied sounds under all these test conditions are produced by "snapping tendons."

But what is evidently thought to be the most crushing blow is the declaration of Mrs. Culver given at length in the Appendix. This person was a connection of the Fox family, and she declared that the Misses Fox told her how it was all done, and asked her to assist them in deceiving the visitors; two gentlemen certify to the character of Mrs. Culver. The answer to this slander is to be found in Capron's "Modern Spiritualism," p. 423. Mr. Capron was an intimate friend of the Fox family, and Catherine Fox was staying with him at Auburn, while her sisters were at Rochester being examined and tested by the committee. Yet Mrs. Culver says it was Catherine who told her "that when her feet were held by the Rochester Committee the Dutch servant-girl rapped with her knuckles under the floor from the cellar." Here is falsehood with circumstance; for, first, Catherine was not there at all; secondly, the Committee never met at the Fox's house, but in various public rooms at Rochester; thirdly, the Fox family had no "Dutch servant-girl" at any time, and at that time no servant-girl at all. The gentlemen who so kindly signed Mrs. Culver's certificate of character did not live in the same town, and had no personal knowledge of her; and, lastly, I am informed that Mrs. Culver has since retracted the whole statement, and avowed it to be pure invention (see Mrs. Jencken's letter to "Athenæum," June 9, 1877). It is to be remarked, too, that there are several important mistakes in Dr. Carpenter's account. He says the "deposition" of Mrs. Culver was made not more than *six* years ago, whereas it was really *twenty-six* years ago; and he says it was a "deposition before the magistrates of the town in which she resided," by which, of course, his readers will understand that it was on oath, whereas it was a mere statement before two witnesses, who, without adequate knowledge, certified to her respectability!*

* Since the MS. of this article left my hands, I have seen Dr. Carpenter's letter in the "Athenæum" of June 16th, withdrawing the charges founded on the declaration of Mrs. Culver, which, it seems, Dr. Carpenter obtained from no less an authority than Mr. Maskelyne! the great conjurer and would-be "exposer" of spiritualism. He still, however, maintains the validity of the explanation of the "raps" by Professor Flint and his coadjutors, who are said to have proved that persons who have "trained themselves to the trick," can produce an "*exact imitation*" of these sounds. This "exact imitation" is just what has never been proved, and the fact that a "training" is admitted to be required, does not explain the sudden occurrence of these sounds as soon as the Fox family removed temporarily to the house at Hydesville. If Dr. Carpenter would refer to better and earlier authorities than Mr. Maskelyne and M. Louis Figuier, he would learn several matters of importance. He would find that Professors Flint, Lee, and Coventry, after one hasty visit to the mediums, published their explanation of the "raps" in a letter to the "Buffalo Commercial Advertiser," dated February 17th, 1851, *before* making the investigation on the strength of which they issued their subsequent report, which, therefore, loses much of its value since it interprets all the phenomena in accordance with a theory to which the reporters were already publicly committed. On this scanty evidence we are asked to believe that two girls, one of them only nine years old, set up an imposture which for a long time brought them nothing but insult and abuse, subjected their father to public rebuke from

This is an example of the reprehensible eagerness with which Dr. Carpenter accepts and retails whatever falsehoods may be circulated against mediums; and it will be well to consider here two other unfounded charges which, not for the first time, he brings forward and helps to perpetuate. He tells us that "the 'Katie King' imposture, which had deluded some of the leading spiritualists in this country, as well as in the United States, was publicly exposed." This alleged exposure was very similar to that of Mrs. Culver's, but more precise and given on oath—but the oath was under a false name. A woman whose name was subsequently discovered to be Eliza White declared that she had herself personated the spirit-form at several stated séances given by the two mediums Mr. and Mrs. Holmes, she having been engaged by them for the purpose; and she described a false panel made in the back of the cabinet by which she entered at the proper time from a bedroom in the rear. But Colonel Olcott, a gentleman connected with the New York daily press, has proved that many of the particulars about herself and the Holmes' stated in Mrs. White's sworn declaration are false, and that she is therefore perjured. He has also proved that her former character is bad; that the photograph taken of "Katie King," and which she says was taken from her, does not the least resemble her; that the cabinet used had no such moveable panel as she alleged; that the Holmes' manifestations went on just the same on many occasions when she was proved to be elsewhere; that she herself confessed she was offered a thousand dollars if she would expose the Holmes'; and, lastly, that in Colonel Olcott's own rooms, under the most rigid test conditions, and with Mrs. Holmes only as a medium, the very same figure appeared that was said to require the personation of Mrs. White.

his minister, and made their mother seriously ill; and that they have continuously maintained the same for nearly thirty years, and in all this long period have never once been actually detected. But there are facts in the early history of these phenomena which *demonstrate* the falsehood of this supposition, but which Dr. Carpenter, as usual, does not know, or, if he knows does not make public. These facts are, firstly, that two previous inhabitants of the House at Hydesville testified to having heard similar noises in it; and, secondly, that on the night of March 31st, 1848, Mrs. Fox and the children *left the house*, Mr. Fox only remaining, and that during all night and the following night, in presence of a continual influx of neighbours *the "raps" continued exactly the same as when the two girls were present.* This crucial fact is to be found in all the early records, and it is surprising that it can have escaped Dr. Carpenter, since it is given in so popular a book as Mr. R. Dale Owen's "Footfalls on the Boundary of Another World" (p. 209). Mr. Owen visited the spot, and obtained a copy of the depositions of twenty-one of the neighbours, which was drawn up and published a few weeks after the events. This undisputed fact, taken in connection with the great variety of sounds—varying from taps, as with a knitting-needle, to blows as with a cannon-ball or sledge-hammer—and the conditions under which they occur—as tested by Mr. Crookes and the Dialectical Committee, completely and finally dispose of the "joint and tendon" theory as applicable to the ascertained facts. What, therefore, can be the use of continually trying to galvanise into life this thoroughly dead horse, along with its equally dead brother the table-turning "indicator"?

The full details are given in Colonel Olcott's "People from the Other World," pp. 425—478.

Another alleged exposure is introduced in the following terms:—"I could tell you the particulars, in my possession, of the detection of the imposture practised by one of the most noteworthy of these lady mediums in the distribution of flowers . . . these flowers having really been previously collected in a basin upstairs and watered out of a decanter standing by—as was proved by the fact that an inquisitive sceptic having furtively introduced into the water of the decanter a small quantity of ferrocyanide of potassium, its presence in the 'dew' of the flowers was afterwards recognised by the appropriate chemical test (a per-salt of iron) which brought out prussian blue."

In his article on the "Fallacies of Testimony," in the "Contemporary Review" of January, 1876, where Dr. Carpenter first gave an account of this alleged exposure, it is stated that "a basin-full of these flowers (hollyhocks) was found in a garret with a decanter of water beside it," that the ferrocyanide was mixed with this water, and that all this was not hearsay, but a statement in writing in the hand of the "inquisitive sceptic" himself. It turns out, however, that this part of the statement was wholly untrue, as we know on the authority of a letter written by the lady of the house, and afterwards published, and Dr. Carpenter now seems to have found this out himself; but instead of withdrawing it wholly (as in common fairness he ought to have done), he still retains it ingeniously modified into an *inference*, but so worded as to look like the statement of a *fact*;—"these flowers having *really* been previously collected in a basin," &c.,—"as was proved"—not by finding them, but by the chemical test! What an extraordinary notion Dr. Carpenter must have of what is "really" proof. Let us, however, look a little further into this matter, of which more is known than Dr. Carpenter adduces, or than he thinks advisable to make public. Dr. Carpenter's informant was a member of the family in whose house the medium was staying as a guest. He had therefore full knowledge of the premises and command over the servants, and could very easily have ascertained such facts as the bringing of a large bunch of hollyhocks, asters, laurels, and other shrubs and flowers into one of the visitors' bedrooms, and whether they disappeared from the room when the lady medium left it previous to the séance. This would have been direct evidence, and easily attainable by one of the family, but none such is forthcoming; instead of it we have the altogether inconclusive though scientific-looking chemical test. For it is evident that the flowers which appear must be brought from somewhere, and may naturally be brought from the shortest distance. If there are flowers in the house, these may be brought—as a baked apple was actually brought when an apple was asked for, according to one of the reports of this very séance;—and if a sceptic chooses to put chemicals with such

flowers or baked apples beforehand, these chemicals may be detected when the flowers or apples are examined. The wonder of such séances does not at all lie in where the flowers are brought from, but in the precautions used. The medium's hands, for instance, are always held (as they were in this instance) yet when thus held the flowers drop on to the table, and even particular flowers and fruits drop close to the persons who ask for them. This is the real fact to be explained when, as in this case, it happens in a private house; and the alleged chemical test has no bearing on this. But here the test itself is open to the gravest suspicion. The person who says he applied it had struck a light in the middle of the séance and discovered nothing. He was then, in consequence of some offensive remarks, asked to leave the room or the séance could not go on; and subsequently high words passed between him and the medium. He is therefore not an unbiassed witness, and to support a charge of this kind we require independent testimony that the chemical in question was not applied to the flowers *after* they appeared at the séance. This is the more necessary as we have now before us the statement in writing by another resident in the house, that some of the flowers were sent to a medical man in the town, and that no trace of ferrocyanide of potassium could be detected. The accuracy of the supposed tests is also rendered very doubtful by another fact. In a published account of the affair in the "Bath and Cheltenham Gazette," endorsed by Dr. Carpenter's informant (in a letter now before me) as being by a friend of his and substantially correct, it is stated that the "same authority" who is said to have "demonstrated the presence of potassium ferrocyanide" on the flowers also examined some sand which fell on the table at the same sitting, and found it to contain salt, and therefore to be sea-sand, and to agree microscopically with the sand from a sea-beach near which the medium had been staying a few days before. This reads very like truth, and looks very suspicious, but it happens that another gentleman who was present at the séance in question took away with him some of the sand for the purpose of subjecting it to microscopic examination; and from that gentleman—Mr. J. Traill Taylor, Editor of the "British Journal of Photography" and an occasional contributor to other scientific journals—I have received the following note on the subject:—"I remember the séance to which you have alluded, and which was held on the evening of August 23, 1874, during the Belfast Meeting of the British Association, which I was attending. At that time, among other bye-pursuits, I was engaged in the microscopical examination of sand of various kinds, and I omitted no opportunity of procuring samples. During my visit to Ireland I obtained specimens from the sea-coast of Counties Down and Armagh, as well as from the shores of Lough Neagh. When the shower of sand fell upon the table during the séance I appropriated a quantity of it for subsequent exami-

nation. The most careful inspection under the microscope satisfied me that it was absolutely identical with some that had been procured from the Antrim coast of Lough Neagh, while it differed in certain respects from that obtained at the sea coast. Having subsequently seen a communication on this subject in the "English Mechanic" (by a writer who, I believe, had not been present at the séance), the purport of which was that the séance sand was similar to some obtained from a part of the sea-coast where the medium had been recently residing, I again subjected these various sands to microscopical examination, only to be confirmed in my previous conclusion. I followed this by a chemical test as follows:—I washed each sample of sand in a test-tube with distilled water, to which I then added a solution of nitrate of silver. A precipitate of chloride of silver was obtained from all the samples of sea-sand, but no precipitate was formed by that which came from Lough Neagh nor by that obtained at the séance, which last, under this chemical test behaved in a manner precisely similar to the Lough Neagh sample. I recollect that the result of this test was my feeling sure that the writer to whom I have alluded had not had the same data as those in my possession for arriving at a conclusion. In about a year after that time I threw away over a dozen different samples of sand, including those to which I have referred, as I required for another purpose the boxes in which they had been kept."

This clear and precise statement demonstrates the untrustworthiness of the authority on whom Dr. Carpenter relies, even if it does not indicate his disposition to manufacture evidence against the medium in question. At all events, with the more complete account of the whole episode now before them, our readers will, we are sure, admit that the evidence is by no means free from suspicion, and is quite insufficient to justify its being used to support a public charge of deliberate imposture. It also affords another example of how Dr. Carpenter jumps at explanations which are totally inapplicable to the facts in other cases, as, for example, to the production of flowers and ferns in my own room, as narrated in my "Miracles and Modern Spiritualism," page 164, and to that in the house of Mr. T. Adolphus Trollope, as given in the "Dialectical Report," pp. 277 and 372, in which case the medium had been carefully searched by Mrs. Trollope before the séance began.

We have now only to notice the extraordinary Appendix of *pieces justificatives*, which, strange to say, prove nothing, and have hardly any bearing on the main questions at issue. We have, for instance, six pages of extracts on early magic, the flagellants, and the dancing mania; followed by four pages about Mesmer; then an account of Mr. Lewis's experiments before the Medical School, Aberdeen, which failed; then eight pages on the effects of *suggestion* on hypnotised patients—effects thoroughly known to every operator, but having no bearing on the case of

persons never hypnotised or mesmerised, and to whom *no suggestion* was made; after this comes ten pages on the planchette, on which no one relies without collateral evidence; and then an account of some foolish clergymen, who thought they had direct proof of Satanic agency; then comes Mrs. Culver's statement (called a "deposition before magistrates" in the text), to which we have already referred; then my own letter to the "Spectator" about Mr. G. H. Lewes's supposed proof of the imposture of Mrs. Hayden; then the oft-told story of Dr. Carpenter's interviews with Foster, from the "Quarterly Review" article; then more of Mr. Braid's "suggestion and expectancy" experiments, —and that is all! Not one solitary piece of careful investigation or unimpeachable evidence in these forty-two pages of what are announced as *pieces justificatives!*

Let us now summarise briefly the results of our examination of Dr. Carpenter's book. We have given a few examples of how he has misrepresented the opinions of those opposed to his theories. Although he professes to treat the subject historically, we have shown how every particle of evidence is ignored which is too powerful to be explained away. As examples of this we have referred, in more or less detail, to the denial by high authorities of the reality of painless surgical operation during the mesmeric sleep; to the "Report of the Royal Académie de Medécine," supporting the reality of clairvoyance and the other higher phenomena of mesmerism; to experiments on clairvoyance, before French medical sceptics; to the evidence of educated and scientific men in Vienna as to the truth of Reichenbach's observations: to the personal evidence of Robert Houdin, Professor Gregory, Dr. Mayo, Dr. Haddock, Dr. Lee, Dr. Ashburner, Dr. Rostan, Dr. Teste, and Dr. Esdaile, as to tests demonstrating the reality of clairvoyance; to the evidence of the Dialectical Committee, of Dr. Lockhart Robertson, Serjeant Cox, Mr. Crookes, and myself, as to motion of solid bodies demonstrably not caused by muscular action; to the evidence of the Dialectical Committee, of the Hon. Robert Dale Owen, Mr. Crookes, and Professor Barrett, as to raps demonstrably not caused by the muscles or tendons of the medium; to the evidence of Mr. T. A. Trollope and myself as to the production of flowers, demonstrably not brought by the medium,— all of which evidence, and everything analogous to it, is totally ignored by Dr. Carpenter. Again, this work, professing to be "scientific," and therefore accurate as to facts and precise as to references, has been shown to be full of misstatements and misrepresentations. As examples we have—the statement that there is no evidence of the mesmeriser's power to act on a patient unconscious of his wish to do so, whereas I have shown that there is good medical evidence of this power; that Reichenbach did not submit his subjects to tests, whereas I have quoted many admirable tests, as well as the independent test-observa-

tions of Dr. Charpignon; that Rutter's magnenometer never acted when the operator did not know the substance influencing it, whereas Mr. Rutter states clearly and positively that it did; that the Royal Academy of Medicine *first* investigated clairvoyance in 1837 and declared it not proved, whereas they first investigated it in 1825, and reported *favourably;* that Professor Gregory was credulous, and took no precautions against imposture, which I have shown to be not the fact. Again we have numerous errors and misstatements (always against the mediums) in the accounts of the Misses Fox and Mrs. Culver, of the alleged "Katie King" exposure, and of the flower-séance chemically exposed. And, lastly, we have the statement, repeated under many forms, that when adequate investigation has taken place, and especially when "trained experts" have been employed, trick or imposture has *always* been discovered. But this I have shown to be the grossest of all misstatements. Surely medical men are "trained experts," and we have nine members of the Royal Academy of Medicine investigating for five years, and a large number of French and English medical men devoting years of enquiry to this subject, and deciding that it is *not* imposture. Are not eminent physicists trained experts, so far ar least as the purely physical phenomena are concerned? But we have Prof. Hare, Prof. Gregory, and Mr. Crookes, who all devoted years of careful investigation to the subject; Prof. Barrett, who has come to it with a fresh and sceptical mind, stored with all the warnings that Dr. Carpenter can give him, and yet declares it to be reality, and neither imposture nor delusion; while another recent convert from extreme scepticism on this subject is Dr. Carter Blake, Lecturer on Comparative Anatomy at Westminster Hospital, who last year wrote me that after months of careful examination he was satisfied that the phenomena called "Spiritual" are thoroughly genuine and worthy of scientific examination,—that he has arrived at this conclusion very slowly, and, referring to his recent investigations, he says—"Every experiment performed has been under the most rigorous test conditions, and the dishonest element which some professional mediums have shown has been rigorously eliminated. Yet, again, professional conjurors are surely "trained experts," and Dr. Carpenter has himself often referred to them as such, but the moment they go against him he ignores them. I have adduced, for the second time, the remarkable evidence of Robert Houdin to the reality of the clairvoyance of Alexis; Mr. T. A. Trollope informs us that another celebrated conjuror, Bosco, "utterly scouted the idea of the possibility of such phenomena as I saw produced by Mr. Home being performed by any of the resources of his art;" and lastly, at Glasgow, last year, Lord Rayleigh informed us that he took with him a professional conjuror to Dr. Slade's, that the phenomena happened with considerable perfection, while "the

conjuror could not form the remotest idea as to how the effects were produced."

We have now concluded what has been a painful task; but in the interests of truth it was necessary to show how completely untrustworthy is the self-appointed guide that the public so blindly follow. By ample references I have afforded to such of my readers as may be so inclined the means of testing the correctness of my charges against Dr. Carpenter; and if they do so they will, I feel convinced, not only lose all faith in his explanations of these phenomena, but will also find how completely ignorant of this, as of most scientific subjects, are those writers in our influential literary press who have, almost without exception, praised this book as a fair and complete exposition of the subject on which it treats.

It also seems to me that an important question of literary morality is here involved. While maintaining as strongly as anyone that new or disputed theories should be subjected to the fullest and severest criticism, I yet hold that this should not involve either misrepresentation or what has been termed the "conspiracy of silence." It is, at the best, hard enough for new truths to make their way against the opposing forces of prepossession and indifference; and bearing this in mind, I would ask whether it is in the interests of human progress and in accordance with right principles, that those who have the ear of the public should put forth, under the guise of impartial history, a thoroughly one-sided and erroneous account of a disputed question. It may be said that errors and misstatements can be exposed, and will only injure the author of them; but unfortunately this is not so. The popular view of a subject like this is sure of a wide circulation, and writers in the daily and weekly papers increase its publicity, whereas few read the answer, and the press decline or refuse to make it known.*

* A striking proof of this statement has been quite recently furnished us. The letter given below was sent by Dr. Slade to Professor E. R. Lankester. It would seem to exhibit, in a high degree, the characteristics of truth, fairness, and charity. No answer was received. The press, moreover, refused to publish it, and the daily press, one and all, *refused to insert it even as an advertisement!*

"PROFESSOR E. R. LANKESTER.

"DEAR SIR,—Dr. Slade having in some measure recovered from his very severe illness, and his engagement to St. Petersburg having been postponed (by desire of his friends there) till the autumn, desires me to make the following offer:—

"He is willing to return to London for the express and sole purpose of satisfying you that the slate-writing occurring in his presence is in no way produced by any trickery of his. For this purpose he will come to your house unaccompanied by any one, and will sit with you at your own table, using your own slate and pencil; or, if you prefer to come to his room it will suit him as well.

"In the event of any arrangement being agreed upon, Slade would prefer that the matter should be kept strictly private.

"As he never can guarantee results, you shall give him as many as six trials, and more if it shall be deemed advisable.

"And you shall be put to no charge or expense whatever.

As the very existence of the press depends on popularity this is inevitable, but it none the less throws a great reponsibility on those who possess this popularity if they mislead public opinion by inaccuracy or suppression of facts.

In his article on "Fallacies of Testimony" Dr. Carpenter, quoting Schiller, says, that the "real philosopher" is distinguished from the "trader in knowledge" by his always loving truth better than his system. If our readers will carefully weigh the facts now laid before them, they will be able to decide how far Dr. Carpenter himself belongs to the first or to the second of these categories.

ALFRED R. WALLACE.

Text-Book of Structural and Physiological Botany. By OTTO W. THOME. Translated and Edited by ALFRED W. BENNETT, F.L.S. London: Longmans and Co.

WE have here a translation of a German work which has found great approbation in its own country, and will probably experience an equally favourable reception in England. It embraces the whole range of elementary botany, and will prove a safe and convenient guide for the student in the earlier part of his career. The Editor, however, very judiciously reminds his readers that in Natural Science "the greater and the most useful part of the student's knowledge must always be acquired in the field, or with the dissecting knife in hand," the use of text-books being merely to put him in the right track for personal research, and to save him from the necessity of re-discovering what others have already

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"You on your part shall undertake that during the period of the sittings, and for one week afterwards, you will neither take, nor cause to be taken, nor countenance legal proceedings against him or me.

"That if in the end you are satisfied that the slate-writing is produced otherwise than by trickery, you shall abstain altogether from further proceedings against us, and suffer us to remain in England, if we choose to do so, unmolested by you.

"If, on the other hand, you are not satisfied, you shall be at liberty to proceed against us, after the expiration of one week from the conclusion of the six or more experiments, if we are still in England. You will observe that Slade is willing to go to you without witnesses of his own, and to trust entirely to your honour and good faith.

"Conscious of his own innocence, he has no malice against you for the past. He believes that you were very naturally deceived by appearances, which, to one who had not previously verified the phenomena under more satisfactory conditions, may well have seemed suspicious.

"Should we not hear from you within ten days from this date, Slade will conclude that you have declined his offer.

"I have the honour to be, sir, your obedient servant,

"J. SIMMONS."

37, Spui-straat, The Hague, May 7th 1877.

observed before him. The real educational value of Natural History, the development of the power of observation, will utterly escape those whose studies are confined to books.

The successive chapters of the work are devoted to a consideration of the cell as an individual; the cell as a member of a group of similar cells; the construction of the plant out of cells; the external form of plants; the life of the plant; special morphology and classification; the changes in the vegetation of the globe during past geological epochs; and botanical geography. The last division is illustrated with a map, showing the twenty-four regions into which the earth is divided by Griesbach in his "Végetation der Erde"—a classification which Mr. Bennett thinks "too unqualified," both with regard to the boundaries between the regions and the characters which distinguish them from one another. It is interesting to compare these regions with the geographical divisions of the animal world, as laid down by Mr. Wallace. We must own to a little surprise at finding Madagascar classed as an "Oceanic Island," along with the Azores, Madeiras, and Canaries.

The chapter on vegetable palæontology gives an accurate but necessarily very brief account of the floræ of bygone ages.

In the section on the "Life of the Plant," the influence of temperature, light, &c., upon vegetation is carefully described.

The work is throughout abundantly illustrated, and will, we hope, prove useful to those real students who seek not to "pass," but to know.

The Geology of England and Wales. A Concise Account of the Lithological Characters, Leading Fossils, and Economic Products of the Rocks; with Notes on the Physical Features of the Country. By HORACE B. WOODWARD, F.G.S., of the Geological Survey of England and Wales. London: Longmans and Co.

MANY as have been the geological works recently put forth in England, there was still, we believe, room for a condensed work of reference on the geology of our own country that should be fully on a level with the present state of the science. This want Mr. Woodward has supplied in what we must pronounce a very satisfactory manner. His treatise is well arranged, comprehensive, accurate, and concise. All unnecessary verbiage has been carefully avoided, so that the student is not placed under the necessity of seeking out the facts he wants amidst a dreary waste of padding. Of speculation there is little. The author declares himself to be what is technically called a "uniformitarian," but he judiciously adds that "in concluding that the physical forces have been the same throughout geological time" we must guard

against supposing their action to have been "always of similar intensity to that of which we have definite proof in the present." He also lays down a limitation which is sometimes conveniently overlooked by cavillers at geology and geologists. "The special province of the geologist," he reminds us, "is only to deal with the Earth after it was in a fit state to receive and support life, when the proportion of land to water was probably much as it is now, and the climate and physical conditions, though ever varying over the same area during the different geological ages, were subject to the same laws and attended by analogous phenomena. . . . To go back to the very earliest history of the Earth, when it was part of a nebulous mass, would be to trespass upon the region of the astronomer, and when we consider its latest history we come upon questions which must be answered by the geographer and the archæologist."

The author evidently accepts, in its general principles, the doctrine of Organic Evolution. He quotes, at any rate without formal disapproval, the opinion of Prof. Huxley, that "the less regard palæontologists pay to the deposit from which fossils are obtained so much the better, for not unfrequently has a new name been given to a known fossil because it has been found in strata where it was previously unknown." The same eminent author even longs for "a new race of palæontologists, utterly ignorant of geology"—a pious wish in which we find ourselves unable to join.

In treating of the economic bearings of geology, Mr. Woodward remarks that "the relation between health and geology is also a point which has in recent years received a good deal of attention, and maps have been published and memoirs written to show the relation between certain forms of disease and geological structure—even between geology and lunacy! It is well known, indeed, that a gravelly, sandy, or chalky soil is more healthy than a clay foundation, because the former are pervious to water and the latter is impervious. On the former there is less consumption than on the latter, as Mr. Whittaker and Dr. Buchanan have clearly demonstrated: the artificial removal of subsoil water has, however, done much to equalise the conditions. Again, the water-supply is a most important subject, for in some small country villages and towns the inhabitants suffer very much from its impurity. Situated, perhaps, on elevated ground, with a good porous soil, they yet suffer because of the disgraceful state of the drainage, the wells being shallow, and the sewage and even the churchyards draining into them. The cause of teetotalism will not find many admirers when it is often the women and children who suffer most from drinking impure water, while the men who take their beer are less subject to disease."

We fear that the sentence last quoted will bring down upon Mr. Woodward the gravest denunciations of the "temperance

party." But we cannot help remarking that it is in districts having a porous subsoil where polluted waters, and other evils arising from sanitary neglect, are most rampant. A stiff compact clay opposes an impassable barrier to the diffusion of cesspool and churchyard drainage. But over chalk and gravel, the cesspool and the well, though separated by an interval of perhaps a dozen yards, are practically identical. In a small town in Kent we even found that the sewage, after removal of the solids by means of subsidence-pits, was allowed to soak down and disappear in the permeable subsoil, which must thus become irredeemably saturated with putrescent matter, and must constantly give off noxious gases and vapours.

As regards the antiquity of the human species, the author admits that it appeared on the earth's stage unquestionably at a date very much earlier than our forefathers imagined. With Mr. Boyd Dawkins, he holds that man was co-existent, in this country and in Western Europe, with the lion, the hairy elephant, and the woolly rhinoceros, and that if his existence can be traced back to or even beyond the Glacial epoch, extending from 240,000 down to 80,000 years ago, a still higher antiquity must be assigned him.

The work is admirably illustrated, and is furnished with a good geological map of England and Wales, with a glossary of technical terms, a synopsis of the animal kingdom, a list of the principal works consulted, and a bibliography of the geology of the English counties.

We consider that Mr. Woodward's work merits almost unqualified commendation.

The Whitworth Measuring-Machine, including Descriptions of the Surface-Plates, Gauges, and other Measuring-Instruments, made by Sir Joseph Whitworth. By T. M. Goodeve, M.A., and C. P. B. Shelley, C.E. London: Longmans and Co. 1877.

We all know how very superior English machinery is to that which is manufactured by many other countries, but we do not always recognise the great share which Sir Joseph Whitworth has had in perfecting the machinery whereby our most accurate work is done. The authors of the work before us state at the outset that "the two principal surfaces of essential importance in the workshop may be distinguished as a 'true plane' and a 'true cylinder.'" The nearest approach to the former is a surface of clean mercury at rest. Sir Joseph Whitworth has devoted much time and thought to the production of perfectly plane surfaces and accurate methods of minute measurement, and his instru-

ments approach to accuracy as nearly as is possible. Messrs. Goodeve and Shelley have given an interesting account of the instruments, accompanied by good plates and woodcuts.

The Theory of Sound. By Lord RAYLEIGH, M.A., F.R.S., formerly Fellow of Trinity College, Cambridge. Vol. I. London: Macmillan and Co.

EVERY student of the mathematical theory of sound has felt the exceeding meagreness of the information on the subject within his reach. The ordinary text-books treat of Sound merely by way of an appendix to Dynamics and Hydro-dynamics, and consequently limit themselves to the most elementary parts of the theory, with scarcely a hint as to the existence of a mass of important recent investigations sufficiently numerous and valuable to require a separate treatment of the whole subject. The book before us is the first instalment of a work intended to remedy this deficiency, and certainly, so far as the range of this first volume extends, achieves a success.

After some valuable preliminary remarks on the ordinary theory of musical notes as due to vibrations, amongst which is to be found a remarkably clear *exposé* (p. 17) of the reasons for supposing the sensation of a simple tone to be due to a simple harmonic vibration, the author proceeds to describe various appliances for investigating the rapidity of the simple vibrations which may be present in any compound vibratory motion, illustrating at the same time the laws of superposition of small motions without interference, and showing very clearly the nature of the interference effect, in a simple case, of the superposition of motions which are so great that the squares and higher powers of the disturbance cannot be neglected—an effect which shows itself in the production of vibrations whose periods are the doubles, sum, and difference of the periods of the originals. The application of the method of generalised co-ordinates to the problem of small vibrations about an equilibrium position is then discussed, and the main part of the work consists of the application of the results to the detailed examination of the vibrations of strings, bars, membranes, and plates. This discussion contains all that is most important in the work of recent investigators, amongst whom Lord Rayleigh holds a foremost place.

The theory of sound is perhaps somewhat uninteresting as yet to the general mathematical reader, but it is full of interest to the investigator on account of the facility and delicacy of the acoustical tests which can be applied to verify theoretical deductions, and the author has taken care to supply this element of interest in this work by constant exhibition of the results of a

comparison between theory and observation. The result is a book which, when sound is applied as an instrument of discovery in molecular physics, will be found to be the most complete grammar we at present possess of the language in which the results of experiment will express themselves.

Elementary Text-Book of Physics. By G. D. EVERETT, M.A., D.C.L. London: Blackie and Son. 1877.

THIS work is intended for school classes, and passes in brief review the principal physical sciences. It is fairly illustrated, and contains many examples and questions to be worked out. It does not appear to us to possess any advantage over some of the very numerous text-books of physics which have been written with the same object during the last ten years.

Handbook of Natural Philosophy. Heat. By DIONYSIUS LARDNER, D.C.L. New Edition, edited by BENJAMIN LOEWY, F.R.A.S. Lockwood and Co. 1877.

THIS is the familiar treatise on Heat of Dr. Lardner, enlarged so as to bring it in accordance with the most modern ideas on the subject. There are only a few new illustrations, but no less than 320 pages of new matter, among which will be found an interesting chapter on Dissociation and the Chemical Effects of Heat, and on the Dynamical Theory of Heat. Mr. Loewy has done his work of editing in a very satisfactory manner, and this work is still in many respects the most generally useful treatise on Heat which exists in our language.

Theoretical Naval Architecture: a Treatise on the Calculations involved in Naval Design. By S. J. P. THEARLE. 2 vols. London: William Collins and Sons. 1877.

THIS work is divided into six parts, which respectively embrace —Calculations relating to (*a*) the forms and dimensions of ships; (*b*) the weights and centres of gravity of ships; (*c*) the strength of ships; (*d*) the propulsion of ships by sails; (*e*) the propulsion of ships by steam-engines; and (*f*) the steering of ships. In great ship-building cities like Glasgow the information herein contained will be eagerly read not only by master-shipwrights, but by the workmen themselves. A separate Atlas

contains sixty well-drawn plates, and a number of tables. The book is likely to become a standard work, and it supplies a want which has been felt.

Elements of Magnetism and Electricity. By JOHN ANGELL. London and Glasgow: William Collins and Sons. 1877.

THIS book forms one of Messrs. Collins's "Elementary Series" of science class books. Since it was first issued it has been very much improved by the addition of new matter, new illustrations, and the examination questions which have been set at South Kensington since 1867 for the "Elementary Stage." The book is very cheap, and is well illustrated; if it has a fault, it is that the author has attempted to crowd too much matter into a small space, and the explanations are sometimes less full than might be desired; but it is quite sufficient for, and suitable to, the students who present themselves for the elementary stage, and it forms a useful class book for schools.

The Winds and their Story of the World. Vis Inertiæ *in the Ocean.* By W. L. JORDAN, F.R.G.S. Hardwicke and Bogue. 1877.

IN the first of these works the author, in discussing the nature of the winds, endeavours to trace a connection between the vortices of Descartes and Newton's law of gravitation. In the second the results of recent oceanic exploration are considered in reference to the *vis inertiæ* of the ocean. The main object of the author seems to be to refute certain assertions of Prof. Huxley and Dr. Carpenter.

What is Vital Force? Or a Short and Comprehensive Sketch, including Vital Physics, Animal Morphology, and Epidemics, to which is added an Appendix upon Geology: Is the Detrital Theory of Geology Tenable? By RICHARD FAWCETT BATTYE. London: Trübner. 1877.

THIS curious book appears to have been written with a view of ventilating certain pet theories of the author. Human progress is so rapid, and scientific workers are so numerous, that it is as much as most people can do to keep *au courant* with the result of real research, upon which all truth must be founded. Hence we do not predict for Dr. Battye's book a very large number of readers, and we are afraid that we cannot class ourselves among them.

Aids to Chemistry, specially designed for Students preparing for Examinations. By C. E. ARMAND SEMPLE, B.A. London: Bailliere, Tindall, and Cox. 1877.

THIS little book forms an useful abstract of the history of the non-metallic elements, and might serve as an example of good lecture-notes to those attending lectures on chemistry for the first time.

The Aquarium; its Inhabitants, Structure, and Management. By J. E. TAYLOR, Ph.D., F.L.S., &c. London: Hardwicke and Bogue.

THE author of this book refers in his opening chapter to "a good deal of quibbling which has taken place respecting the word 'Aquarium,'" but he considers that it has now "gained its ground," and has "passed out of the regions of philology into that of common parlance." This is all very true as regards the present, but we fear Dr. Taylor has overlooked the probability of more serious discussions in the future. We can imagine in the year of grace 2877, if the world, our modern civilisation, and the English language, should last so long, some Dr. Dryasdust reading a paper on the primitive character of aquaria, and exciting the incredulity of the public by maintaining that they were originally destined to promote the study of natural history, and once contained fishes other than those facetiously characterised as "loose." It is but too probable that these establishments, whilst increasing in number and in popularity, are being developed as general places of amusement in which natural history will play a part about as important as did the poor halfpenny-worth of bread in Falstaff's tavern bill. Dr. Taylor, however, addresses himself to the lovers of the aquarium in its original sense, as a place where the habits and development of aquatic animals may be studied, and where valuable observations, economical as well as theoretical, may be and have been made.

The first two chapters of the book are devoted to a history of aquaria with a notice of such as have acquired especial reputation. Among these a prominent place belongs to the Naples Aquarium or zoological station founded by Dr. Dohrn, with the assistance of Mr. Lloyd and that of Professor Agassiz, at Penékese Island, near New York. These establishments are *bona fide* places for thorough scientific work, fitted up with dissecting rooms, physiological laboratories, &c., and are open to receive students and others desirous of conducting researches in marine zoology. It is scarcely needful to say that "rinking," promenade concerts, and theatricals form no part of the programme. Such truly scientific aquaria are much needed at such

places as Ceylon, Mauritius, Labuan, near Sydney, in the West Indies, &c., and if placed under suitable management could not fail to yield a rich harvest of valuable results. Are there no wealthy men at home or in the colonies able and willing to follow the example of that true "merchant prince" who defrayed the expense of the great Penékese Aquarium?

In the next chapter Dr. Taylor enters upon the "principles of the aquarium," which he recommends as a feature in private houses valuable as a source of comfort and enjoyment to invalids and sedentary persons, as a means of cherishing a love for natural history and as a means of moral education for children. This latter function it fulfils by neutralising that fondness for inflicting pain and death which is unfortunately so deeply rooted in the English nature. The author maintains that a fresh-water "aquarium properly constructed and peopled with proper inhabitants gives very little trouble indeed." He cautions beginners against the common error of overstocking their aquaria and of exposing them to an excessive amount of light, the result of which is the development of a green film of *Algæ* over the glass and over the water-plants. A well-balanced aquarium does not require a frequent change of water. In succeeding chapters an account is given of the amphibians, fishes, insects, and plants best suited for stocking a fresh-water tank, and an especial and interesting section is devoted to the "aquarium as a nursery for the microscope," or rather for microscopic objects.

The author then turns to the construction and management of salt water or marine aquaria, which present undoubtedly greater difficulties, and require to be on a larger scale, but which, on the other hand, offer a much wider scope.

The work must be pronounced to be clearly and ably written, well and abundantly illustrated, and will doubtless draw increased attention to aquaria, and thus render them more serviceable to science.

SCIENTIFIC NOTES.

THE western states and territories of the American Union have suffered very seriously from the ravages of noxious insects, among which the locust, or hopper, as it is familiarly called, holds a bad pre-eminence. Under these circumstances it is very fortunate that the American Government and people do not, like so many nearer home, look upon entomology as a frivolous and useless pursuit, but have of late years given decided encouragement to its cultivation. A commission has been appointed to investigate and report on the best means of combatting the locust. The commissioners, Messrs. C. V. Riley, A. S. Packard, Jun., and C. Thomas, have all "made their mark," and the public may rest assured that their recommendations will be based upon an accurate knowledge of the habits of the destroyer. The United States Commissioners propose, as the means of destroying the eggs and newly hatched young, ploughing, with subsequent harrowing and rolling. For the first eight or ten days after hatching, and in the mornings and evenings subsequently, they are sluggish, huddle together, and may be easily driven into rows of burning straw, or into ditches about 2 feet wide by 2 feet deep, and with steep sides. Wider ditches permit their escape more readily, except the depth be increased likewise. Various kinds of drag-nets and bags, to be drawn by hand or horse-power, have also been used with great effect. With one of these, in Minnesota, from eight to twelve bushels of pupæ have been taken daily. The Commissioners finally insist strongly on the protection of all insectivorous birds—a very judicious recommendation.

The records of the "United States Geological and Geographical Survey of the Territories" always contain much valuable information. No. 4 of vol. ii. of the Bulletin comprises five important memoirs, namely, "Notes on the Geology of North-eastern New Mexico," by O. St. John; "Sexual, Individual, and Geographical Variation in Leucosticte Tephrocotis," by J. A. Allen; "Geographical Variation among North American Mammals, especially in respect to Size," by J. A. Allen; "Descriptions and Illustrations of Fossils from Vancouver's and Sucia Islands, and other North-western Localities," by F. B. Meek; and "Note on the New Genus Uintacrinus," by F. B. Meek. Mr. Allen after a careful examination of the series of skulls of the North American mammalia contained in the National Museum—a collection which he may well term "magnificent," amounting as it often does to 80 to 100 specimens of a single species—has been greatly struck with the different degrees of variability exhibited by representatives of species and genera even of the same family. In the wolves and foxes the variation in size with latitude amounts to 25 per cent of the average size of the species, while in other species of the *Feræ* it is almost *nil*. The common supposition, however, that the size of a species decreases with a decrease in the latitude cannot be admitted as a general law, since, in some forms, there is a well-marked increase as we proceed southward. He expresses the connection of size with geographical distribution in the following laws:—(1.) "The maximum physical development of the individual is attained where the conditions are most favourable to the life of the species." (2.) "The largest species of a group are found where the group to which they severally belong reaches its highest development, or where it has what may be termed its centre of distribution." (3.) "The most 'typical' or most generalised representatives of a group are found also near its centre of distribution, outlying forms being generally more or less 'aberrant' or specialised." The first portion of vol. iii. is a description of new genera and species of dipterous insects from the region west of the

Mississippi and especially from California, by C. R. Osten Sacken. The author expresses himself convinced that "the western fauna is essentially *one*, and that many of the characteristic forms of California sooner or later will turn up in Colorado." He has traced unexpected analogies and coincidences between the fauna of California and those of Europe, Chili, and even Australia, and, on the other hand, has found unforeseen differences from the fauna of the Atlantic States. His final summary of results is exceedingly instructive, and must be regarded as a most valuable contribution to animal geography. Contrary to his former belief he finds that the Rocky Mountains do not form a natural boundary for a distinct entomological fauna. The true boundary is an imaginary line, which some place about longitude 98° W., and others about 100°, and to the west of which agriculture becomes precarious without the aid of artificial irrigation, owing to the intense dryness of the summers. In this region the insects, like all other organisms, have to adapt themselves to the prevailing meteorological conditions. This accounts for the prevalence of the Heteromera among beetles. These insects have a remarkable power of resisting desiccation, owing to the toughness of their integuments, and being nocturnal they escape the action of the sun. Like the *Carabidæ* of the same region, also nocturnal, they have usually a black colour. The same dryness of soil and climate fosters the fossorial Hymenoptera, and the nests of these are again infested by *Melœdæ* among Coleoptera, and *Bombylidæ* among Diptera. Similar climatic conditions occur in the eastern hemisphere in the Mediterranean region and Central Asia, and there also the same forms of life prevail. The author, however, holds that the resemblance between the West American and the Mediteranean faunæ is not a relationship, but a mere analogy, due to similar climatic conditions. Of insects the same families prevail, but not necessarily the same genera. A totally different case is the resemblance between the Californian fauna and that of Northern and Central Europe. Here, instead of similarity, there is a striking difference in climate. That, therefore, European species should be found in California, Texas, and Colorado and yet be wanting in the Atlantic States, points to "some hidden genetic connection between the faunæ of Europe and of Western America." Thus the common magpie, unknown in the Eastern States, is not uncommon in California. Among insects the author furnishes many curious instances of this similarity. Sometimes genera are common to the two regions, sometimes species, and sometimes, again, Californian forms find their nearest representatives in Europe. Our English *Papilio Machaon* is almost identical with the *P. Zolicaon* of California. *Plusia gamma*, one of the most abundant European moths, is very common in California, but does not occur east of the Mississipi. The distribution of the dipterous genus *Apiocera* is most remarkable; hitherto it has been only found in Chili and Australia, but the author describes a species from California. Mr. P. R. Uhler's report on the insects collected by himself in 1875, and on the Hemiptera collected by Dr. Packard, Jun., is also important. He describes insect life as very abundant in Colorado. "On the open commons of the city of Denver," he writes, "I was delighted to see large patches of showy flowers, and to observe how certain insects of similar colours flew to and rested upon them. Very conspicuously was this the case with a delicate blue Lupin. Two species of the little blues, *Lycæna melissa* and *L. rapahæ*, settled upon these flowers, and when at rest were very difficult to recognise." The swarms of grasshoppers—one of the greatest pests of Western America—attracted the author's attention. He noticed that the females were much more numerous than the males—in the cases where he was able to count even to the extent of ten to one. Into the lists of insects captured or seen, and the detailed descriptions of the Hemiptera, we cannot of course enter. The same remark must apply to Dr. A. S. Packard's descriptions of Dr. Thorell's collection of spiders from the Colorado territory.

From the records of the "Geological Survey of India" we learn with regret the resignation of Dr. Oldham, owing to declining health. As Superintendent of the Geological Survey of India and President of the Asiatic Society of Bengal, he has "made his mark" and rendered the most important services to

science. In part 2 of vol. ix. of these Records Mr. Lydekker gives an interesting description of a cranium of *Stegodon Ganesa*, with notes on the sub-genus and on allied forms. From the number of species and genera found in the Siwaliks and other Indian strata he considers that India was the original home of the Proboscidians. *Elephas, Mastodon, Deinotherium, and Tapirus* are all found fossil in that country. The migration probably took plase from thence, where all the sub-genera had arisen long before the Siwalik times. "*Loxodon planifrons* or an unknown allied species might have travelled westwards and given rise to *Loxodon meridionalis* of the English 'Forest Beds,' and subsequently to the living *Loxodon africanus*. *Euclephas* may first have given rise to the Siwalik species, from which again sprang the Narbada species and the living *Euclephas indicus*, and, on the other hand, to another branch which travelled over Asiatic Russia and thence to Europe, producing the Mammoth (*E. primigenius*) and the other European species." "*Mastodon*, as having the widest distribution—Europe, Asia, and America—as well as from being the most generalised type of the family, may well be considered as the most ancient form of the group; its earliest occurrence in India is in the supra-nummulitic beds of Sind and Kach, and its latest existence was probably in the marshes of the Ohio, where it not unlikely lived down to the human period. It is the only American representative of the family, and its migration may well have taken place from India westward. Mastodon was the first of the elephants to die out in India, it being unknown after the Siwalik period." Part 3 includes a paper on "The Age of some Fossil Floras of India," by Ottocar Feistmantel; a note on "The Geological Age of Certain Groups Comprised in the Gondwána series of India, and on the evidence they afford of distinct Zoological and Botanical Terrestrial Regions in Ancient Epochs," by W. T. Blanford, F.R.S.; a memoir on "The Fossiliferous Strata at Matéri and Kotá," by T. W. H. Hughes, F.G.S.; and notes on the "Fossil Mammalian Faunæ of India and Burma," by R. Lydekker, B.A. Mr. Blanford concludes "that the faunas and floras of distant lands varied in palæozoic and mesozoic times, as they do at the present day far more than the fauna of the seas; in short, that there were distinct zoological and botanical provinces, and that evidence founded upon fossil plants of the age of rocks in distant regions must be received with great caution, and that such evidence is certainly in some cases opposed to that furnished by the marine fauna." Mr. Lydekker, in summing up the fossil mammalia of India, finds that all the species found in the Indian tertiaries below the Nerbudda beds are extinct. The numerical relations of the genera are as follows:—

Extinct	25
Peculiar to Indian tertiaries	14
Common to Indian and European tertiaries	26
Common to fossil and living Indian faunæ	17
Common to Indian tertiaries and modern Africa ..	12
Common to Indian tertiaries and modern Europe ..	8

"The greatest number of genera common to any two periods occur in the tertiaries of Europe and India; next to them the greatest common number is found in the living and fossil Indian faunæ; thirdly, a small number of genera is common to the extinct fauna of India and the living fauna of Africa; a few genera are common to the extinct Indian fauna and the modern European fauna; while a larger number of genera are common to the living faunæ of India and Africa." These results, the author considers, "appear clearly to point to some former connection by land between the continents of India, Africa, and Europe." A land connection between the two former regions stretching across the present Indian Ocean has been named Indo-oceania in a recent paper by Mr. H. F. Blanford ("Quarterly Journal of the Geological Society of London," November, 1875), and is known among other investigators as Lemuria. In those days it must be remembered that the peninsula of India was not connected with Central Asia, but was separated from it by a deep Eo-Miocene sea. The connection between India and Europe was of course by way of Africa.

XUM

Part 1 of vol. xii. of "Memoirs of the Geological Survey of India" contains an account of the geological features of the South Mahratta country and of the adjacent districts. The author, Mr. R. Bruce Foote, F.G.S., describes the various formations from the gneiss and the associated intrusive rocks to the subaërial formations and soils. He mentions a small natural lake near Sanageh, the only one in Southern India, but remarks—"There is one other reservoir of fresh water which I should unhesitatingly regard as a true lake if glacial phenomena were admissible in the peninsula of India." On this important point, the former glaciation of India, we do not find here any evidence. On the Shervaroy Hills peat forms largely at an elevation of above 4000 feet, and in the Wynád at much lower levels. This is an important fact, since the formation of peat within the tropics, except on very high mountains, has been declared impossible. In the lower iron-clay of Sadda caves occur, said to be very extensive: they have not, however, been explored. The economic geology of the district presents no striking features. They afford nothing of value except building-stone, some of very fine quality, and iron-ore, the demand for which is diminishing owing to the scarcity of fuel. There is an earthy form of peroxide of manganese found among the dolomite of Bhimgarh, and gold occurs in some of the streams flowing into the upper part of the Malprabha, but the yield is exceedingly small. The author failed in getting an appreciable quantity of gold in a number of carefully-selected samples of sand and gravel collected in promising places in the bed of the stream. The work is illustrated with sections and characteristic views of the scenery, many of which present very remarkable features.

Physics.—Continuing his researches on the "Molecular Pressure" theory of the repulsion resulting from radiation, Mr. Crookes, F.R.S., has constructed an instrument in which a movable fly is caused to rotate by the molecular pressure generated on fixed parts of the apparatus. This instrument, which is called the Otheoscope (ωθέω, I propel), he described to the Royal Society on April 26, 1877. While the glass bulb is an essential portion of the machinery of the radiometer, without which the fly would not move, in the otheoscope the glass vessel simply acts as a preserver of the requisite amount of rarefaction. Carry a radiometer to a point in space where the atmospheric pressure is equal to, say, one millimetre of mercury, and remove the glass bulb; the fly will not move, however strong the incident radiation. But place the otheoscope in the same conditions, and it will move as well without the case as with it. The following is a list of the otheoscopes Mr. Crookes has already made, together with some new experimental radiometers, which were exhibited at the Soirée of the Royal Society in May last:—

1. *Otheoscope.*—A four-armed fly carrying four vanes of thin clear mica is mounted like a radiometer in an exhausted glass bulb. At one side of the bulb a plate of mica blacked on one side is fastened in a vertical plane, in such a position that each clear vane in rotating shall pass the plate, leaving a space between of about a millimetre. If a candle is brought near, and by means of a shade the light is allowed to fall only on the clear vanes, no motion is produced; but if the light shines on the black plate, the fly instantly rotates as if a wind were issuing from this surface, and keeps on moving as long as the light is near.

2. *Otheoscope.*—A four-armed fly carries roasted mica vanes, and is mounted in an exhausted glass bulb like a radiometer. Fixed to the side of the bulb are three plates of clear mica, equidistant from each other in a vertical plane, but oblique to the axis. A candle brought near the fixed plates generates molecular pressure, which falling obliquely on the fly, causes it to rotate.

3. *Otheoscope.*—A large horizontal disk, revolving by the molecular disturbance on the surface of inclined metallic vanes, which are blacked on both sides in order to absorb the maximum amount of radiation.

4. *Otheoscope.*—Inclined aluminium vanes driven by the molecular disturbance from the fixed blacked mica disk below, blowing (so to speak) through them.

5. *Otheoscope.*—A large horizontal coloured disk, of roasted mica, driven by inclined aluminium vanes placed underneath it.

6. *Otheoscope.*—A bright aluminium disk cut in segments, and each segment turned at an angle, driven by a similar one below of lampblacked silver.

7. *Radiometer.*—A vertical radiometer, made with eight disks of mica blacked on one side, and the whole suspended on a horizontal axis which works in two glass cups. The motion of the radiometer is assisted on each side by driving vanes of aluminium blacked on one side.

8. *Radiometer.*—A vertical turbine radiometer, the oval vanes of roasted mica blacked on one side.

9. *Radiometer.*—A spiral radiometer of roasted mica blacked on the upper side.

10. *Radiometer* of large size, showing great sensitiveness.

11. *Radiometer.*—A two-disk radiometer, the fly carrying roasted mica disks blacked on one side; in front of each black surface is fixed a large disk of thin clear mica. The molecular disturbance set up on the black surface, and streaming from it, is reflected in the opposite direction by the clear plate of mica, causing the fly to move abnormally, *i.e.*, the black surface towards the light.

12. *Radiometer.*—A two-disk radiometer, the fly carrying roasted mica disks blacked on one side, similar to No. 11, but with a large clear disk on each side. The molecular disturbance, prevented from being reflected backwards by the second clear disk, is thus caused to expend itself in a vertical plane, the result being a total loss of sensitiveness.

13. *Radiometer.*—A two-disk, cup-shaped, aluminium radiometer, facing opposite ways; both sides bright. Exposed to a standard candle 3·5 inches off, the fly rotates continuously at the rate of one revolution in 3.37 seconds. A screen placed in front so as to let the light shine only on the convex surface, produces repulsion of the latter, causing continuous rotation at the rate of one revolution in 7·5 seconds. When the convex side is screened off, so as to let the light shine only on the concave, continuous rotation is produced at the rate of one revolution in 6·95 seconds, the concave side being apparently attracted. These experiments show that the repulsive action of radiation on the convex side is about equal to the attractive action of radiation on the concave side, and that the double speed with which the fly moves when no screen is interposed is the sum of the attractive and repulsive actions.

14. *Radiometer.*—A two-disk, cup-shaped, aluminium radiometer, lampblacked on the concave surfaces. In this instrument the usual action of light is reversed, rotation taking place, the bright convex side being repelled, and the black concave attracted. When the light shines only on the bright convex side, no movement is produced, but when it shines on the black concave side, this is attracted, producing rotation.

15. *Radiometer.*—A cup-shaped radiometer similar to the above, but having the convex surfaces black and the concave bright. Light shining on this instrument causes it to rotate rapidly, the convex black being repelled. No movement is produced on letting the light shine on the bright concave surface, but good rotation is produced when only the black convex surface is illuminated.

16. *Radiometer.*—A multiple-disk, cup-shaped, turbine radiometer, bright on both sides, working by the action of warm water below and the cooling effect of the air above.

17. *Radiometer.*—A four-armed metallic radiometer with deep cups, bright on both sides.

18. *Radiometer.*—A four-armed radiometer, the vanes consisting of mica cups, bright on both sides.

19. *Radiometer.*—A four-armed radiometer, having clear mica vanes. The direction of motion being determined by the angle formed by the mica vanes with the inner surface of the glass bulb.

An important improvement in the production of the electric light has

been made by M. Paul Jablochkoff, an officer in the Russian engineering service. M. Jablochkoff's invention is called the Electric Candle, and is said to be devoid of all those appliances and restrictions which have rendered the application of the ordinary electric light to general lighting purposes an impossibility. The electrodes employed are composed of two small slips of carbon placed side by side, insulated by a piece of *kaolin*. Kaolin, in its solid state, is an insulator offering high resistance to the electric current, but which, under the influence of a powerful electric current, becomes heated and liquefies, in which state it is no longer an insulator, but a conductor offering a slight resistance to the current, which, when passed through it in this condition, affords a light, soft, steady, and brilliantly white, although it may be coloured by mixing with the kaolin the colour required. No mechanism is required to regulate this light, which once set up continues to burn during the passage of the current until the carbons are consumed, when they are replaced by others. The electrical arrangement consists of an ordinary magneto machine sending positive and negative currents alternately. From this machine radiate wires by which the current evolved is conveyed to the buildings, or points, at which it is required for use. The illuminating arrangement is put in circuit with these wires, and on the current traversing the carbon electrodes, it fuses the kaolin and produces the light. Thus, given a means of producing the necessary electric current, any number of lights may be obtained from the same electro-motor; each dependent upon itself and all entirely independent of each other. Any one light may be brought into use at pleasure, and extinguished when required, by connecting, or disconnecting, the wires in connection with them; whilst a light consumed may be replaced by another with equal ease. A series of experiments illustrating the lighting capabilities of this invention took place on June 15, at the West India Docks. The electric power was produced by one of the Paris *Alliance* Company's magneto-electric machines, with thirty-two horse-shoe magnets of seven plates each, and worked by a small agricultural engine of about eight horse-power. A yard 150 feet by 70, and covered with an awning, was well lighted with four lamps, mounted on posts about 15 feet high, each of which were said to be equal to one hundred gas-lights, but the size of the gas-light was not specified. The lights were toned down by opal glass globes. Pearl type was legible at any part of the covered area. After burning for about twenty minutes the lights were extinguished, and four gas-lamps, with four powerful burners to each, were turned on, evidently with the intention of showing the difference between the orange colour of the gas-flame and the pure white of the electric lamp. The company then adjourned to a large warehouse at the top of an adjacent building, measuring about 50 yards long by 25 yards wide, which was lighted from the outside by three electric candles without any intervening globes. Two of these candles were placed at the side and one at the end, but being only breast high the shadows of persons passing in front of them greatly interfered with the experiments tried. One of the objects of this portion of the trial was to ascertain whether this light could be used for sampling various descriptions of produce and merchandise. Several experts were present, but owing to the position of the light being horizontal instead of vertical, there semed to be some doubt as to its value in the case of samples of coffee, grain, pepper, and similar commodities, inasmuch as the strong shadow cast horizontally by the individual grains of the produce under examination interfered materially with their colour—the particular tint of a coffee berry, for instance, being an important factor in the estimation of the value of a sample. It was far otherwise with a nnmber of samples of coloured alpaca goods. The most difficult colours to judge of by gas-light, or during foggy weather, are dark olive greens, puces, and blues. Next to these come the lightest shades of straw-yellow and cream-colour. In the first case the colours are not to be distinguished from black, and in the second from white; but under the electric light the darkest Navy blues and the lightest greys came out in their true tints, even to the eyes of the uninitiated. The company then adjourned to the quay below, alongside which a large barque had been moored. Here the practicability of lighting ships' decks and holds, and the adjoining wharfs, was

most satisfactorily demonstrated, as well as the portability of the light itself. The amount of heat given off by M. Jablochkoff's candle is comparatively small, the glass globe of one of the lanterns used for lighting up the ship having been found to be only just comfortably warm after having been lighted for twenty minutes. The last experiment tried was, scientifically speaking, the most interesting of the whole, demonstrating as it did that M. Jablochkoff has succeeded in entirely doing away with the necessity for using carbons for the electric light. His newest form of candle consists of a thin plate of his kaolin composition, about 1½ inches long by 1 inch broad, and about $\frac{1}{20}$th inch thick. The sides of the plate are inserted in grooves cut in the wires forming the electrodes of the battery, which project very slightly above the top of the plate. In order to light this new form of candle a bridge of ordinary graphite is carried along the top edge of the porcelain plate. The graphite become incandescent, causing the porcelain to melt, which then becomes a conductor. The graphite gradually disappears, and the melted portion of the porcelain becomes incandescent, gradually vapourising at the rate of a millimetre an hour. The light given out by the porcelain seems softer, mellower, and much more constant and steady, than that given off by the combination of carbon and porcelain: indeed, after five minutes' examination with black spectacles, we failed to discern anything more than a barely perceptible start at distant intervals.

A singular case of the production of heat has been communicated to the French Academy of Sciences by M. J. Olivier. A square rod of steel, 80 centimetres in length and 15 millimetres square, is grasped firmly by both the hands of the operator, one of the hands being placed in the middle of the rod, and the other at one end. The free extremity is strongly pressed against an emery wheel revolving very rapidly. After a few minutes the extremity thus rubbed becomes strongly heated: the hand placed in the middle of the bar does not experience any feeling of heat, but the one at the ether extremity is heated to such an extent that the operator is compelled to let go.

The official appreciation of scientific knowledge in our Colonies is admirably shown by the official reports recently issued by Colonel A. Brunel, the Commissioner of Inland Revenue for Canada. These are reports on weights and measures, and on the analysis of gas and food. A sure evidence of the continued material progress of a country is shown by its appreciation of scientific knowledge as applied to accurate measurement. For the maintenance in Canada of the British standards of length and weight, and for the issue to the local authorities, as well as to chemists and physicists there, of precise copies of these standards, there has been established at Ottawa a department under proper scientific direction, and provided with apparatus of the highest class. This department has also the appointment and control of the Inspectors whose duty it is to inspect the trade weights and measures. These numerous Inspectors are provided with weighing and measuring apparatus of the best design. From Colonel Brunel's report it appears that—"The analysis of gas and food is being carried out in the Dominion in accordance with the latest scientific experience. In Canada, as elsewhere, condiments, coffee, and milk appear to be largely and unwholesomely adulterated. Quinine wine, which is an article of great demand in Canada, is found as sold to be a highly alcoholised wine containing gentian and *nux vomica*, with 20 per cent of alcohol, and is therefore a powerful stimulant instead of being a simple tonic. The testing of gas supplied for lighting and heating purposes is also the duty of the Department at Ottawa, and for this purpose chemists have been appointed, and a large quantity of photometric apparatus obtained from Mr. Sugg, of Westminster. The law appears, however, scarcely to have come yet into active operation, and we regret to see that it is at present deficient in relation to the inspection of the illuminating power of gas. We are glad to find that Colonel Brunel has been so well supported in the difficult task he has initiated, and we trust that his Department may be imitated in other of our Colonies and Dependencies.

MICROSCOPY.—The "Monthly Microscopical Journal" of May contains a valuable paper by Mr. Thomas Palmer, B.Sc., "On the Various Changes Caused in the Spectrum by Different Vegetable Colouring Matters." The paper is accompanied with several spectrum charts, and contains a detailed account of the processes used in preparing the various colouring matters for examination, and also accurate measurements, reduced to wave lengths, of the positions of the various absorption bands. Mr. Sorby, in his remarks on the paper, stated that vegetable colouring matters are much more complex than is generally supposed; most of them are undoubted mixtures of two or three kinds of matters, and even the chlorophyll, the green colouring matter alone, is composed generally of two green matters, which exist separately in certain plants. The line it would be most important to carry out would be, what were the chemical differences which gave rise to these changes in the spectra.

"Notes on Inclusions in Gems, &c.," by Isaac Lea, LL.D. This paper, published in the "Proceedings of the Academy of Natural Sciences, Philadelphia," and reprinted in the "Monthly Microscopical Journal," vol. xvii., p. 198, may be consulted with advantage by all persons interested in this subject. It contains the history of the cavities in crystals and their contents, from the paper by Sir Humphry Davy, published in the "Philosophical Transactions," 1822, to the present time; a full list of references is given, and it forms a valuable contribution to the bibliography of this interesting subject.

An ingenious modification of the achromatic condenser has recently been constructed by Messrs. Beck. It consists of an achromatic combination mounted in the usual fittings; over this slides a cylinder upon an eccentric rotating axis; the disk closing the upper end contains a series of holes, which can be brought accurately one by one over the lenses of the condenser by the rotation of the fitting; these apertures carry a series of lenses, by which a modification of the illuminating pencil is obtained. Lenses, achromatic or not, may be used to alter the aperture and focal length; spot lenses, central or marginal stops can all be employed with the greatest facility, rendering the variety of illumination attainable almost infinite.

A new and simple oblique illuminator has been contrived by the Rev. Lord S. G. Osborne, M.A., which he calls the "Exhibitor." The foundation consists of a "Darker" stage, as used for carrying selenite plates in polarising. Two counter sinkings are turned in the revolving ring, the top one rather larger in circumference and shallower than the lower one. Into the lower sinking is dropped a disk of blackened metal, with a small hemispherical lens mounted in the centre. Into the upper sinking are placed thin metal disks with certain apertures made in them; the front of these disks is just level with the face of the stage, the back close to the front of the disk holding the small lens. A fine screw is cut into the back part of the revolving ring, coming up just below the lower counter sinking; into this screws a brass ring carrying another hemispherical lens of the full size of the aperture. The screw movement has a milled edge, permitting the distance between the lenses to be regulated. The apertures may be made as found useful; one very successful consists of No. 1, a fine slit, in length about the aperture of the upper lens *in the centre;* No. 2, a similar aperture a little way, say not quite its own length from the centre; Nos. 3 and 4, a pin hole and a triangle, also a little out of the centre; these may all be cut in one disk. On the stage there are two steel springs for holding the sides, giving the means of shifting them in any direction. Abraham's achromatic prism is to be preferred to the mirror, and the flame of the lamp turned *edgeways*.